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*Wm. Leyman M.D.*  
*June 20<sup>th</sup> 1858*

ON FECUNDITY, FERTILITY  
AND STERILITY







FECUNDITY  
FERTILITY, STERILITY  
AND ALLIED TOPICS

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## PREFACE.

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IN publishing this volume I have to apologise for its many imperfections, arising from its being rather hurriedly thrown together than carefully composed. For such imperfections I can claim the excuse of a busy professional man—that it has been entirely written in scattered *horæ subsecivæ*. Of its positive faults, unknown to me, but probably existing, I must bear the full blame; only, in anticipation, I express my regret that they should find their way into my work.

I have pleasure in calling special attention to Part VI. of the volume, the valuable contribution of Professor TAIT, made in the most generous manner, and at much inconvenience to himself.







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## PART I.

### ON THE VARIATIONS OF THE FECUNDITY AND FERTILITY OF WOMEN ACCORDING TO AGE.

It has from the earliest times been a matter of philosophical inquiry how much influence the age of women, at the time of commencing to live in wedlock, exerts upon their fertility, and opposite opinions on this point have been embraced by authors of note. The various questions connected with this topic have always been unsatisfactorily treated, on account of the insufficiency and inaccuracy of the data used to settle them, when facts and not mere impressions were the foundations of argument. In the following pages I have attempted to introduce some degree of exactness into the subject, and have used a considerable mass of figures as the almost exclusive basis of my conclusions.

In 1855, when the systematic registration of births in Scotland was established, the schedule in use exacted from the public a variety of interesting details in connection with each return—a circumstance which gives to the registers for that year an extraordinary value. For, in consequence, I believe, of numerous complaints regarding the irksome labour of filling up the document, it was discontinued, and a much less compre-



hensive schedule has been in use ever since. It is from the registers of births for 1855 that I have extracted almost all the data which have yielded the results I am now about to communicate. Similar data cannot be found in the subsequent registers, nor, so far as I know, in any other registers whatsoever.

The exigencies of time, labour, and expense, constrained me to restrict the number of births to be operated on, within moderate limits; and I selected Edinburgh and Glasgow, with their 16,593 children legitimately born in 1855, as the field of operations. It is needless to enter fully upon the reasons of my selecting the conditions of legitimate birth in Edinburgh and Glasgow; my only object was to secure as much as possible of accuracy and completeness in the filling up of the schedules—an object quite incompatible with the inclusion of the data derived from mothers of illegitimate children. It must be noted, that legitimate births, as registered, include only births of living children at the full term of pregnancy, or near it.

The well-known difficulty of handling statistics without infringement of the rules of logic has made me very cautious in my progress in this investigation, and I am all the more bound to be careful, because it will be necessary, in connection with my present topic, to point out great errors made by authors who have entered upon it. But, although I trust no fault will be found with my mode of reasoning, I have to admit the existence of some comparatively few and unimportant errors in the details given in the registers.



The chief of these will be stated in connection with the tables to be brought forward. So far as I know, the errors are all in the original registers; in the elaboration of the details thence derived I have spared nothing that could insure accuracy; and must here mention my obligations to the various intelligent and assiduous gentlemen who have assisted me in the work, especially the late Dr. Craig, Drs. Anderson and Linton, and Messrs. Brown and Slater.

The first part of my investigations is confined to the determination of the comparative fertility or productiveness and fecundity of women at different ages. It is necessary, in order to avoid confusion, here to establish some amount of distinction, which I shall maintain as I go on, between fertility or productiveness and fecundity. By fertility or productiveness I mean the amount of births as distinguished from the capability to bear. This quality of fertility or productiveness is interesting chiefly to the statistician or the political economist. When a population is the subject of consideration it does not even involve the capability of every individual considered to bear, nor even the conditions necessary for conception. By fecundity I mean the demonstrated capability to bear children; it implies the conditions necessary for conception in the women of whom its variations are predicated. This quality of fecundity is interesting chiefly to the physiologist and physician. In short, fertility implies fecundity, and also introduces the idea of number of progeny; while fecundity simply indi-



cates the quality without any superadded notion of quantity.

In discussing the subject of comparative fertility and fecundity at different ages, I may incidentally afford means for estimating the degree of fertility or fecundity of different ages; but I wish it to be distinctly understood that I have not proposed to myself, in this part, the consideration of the actual degree or amount of fertility or fecundity at any age, but chiefly the variations of fertility or fecundity at different ages as compared with one another. The fertilities of mothers of different ages I shall take up in a subsequent part.



## CHAPTER I.

THE ACTUAL FERTILITY OF THE FEMALE POPULATION AS  
A WHOLE AT DIFFERENT AGES.

THE first law which I propose to establish has reference to the ages of the mothers of legitimate children. In Edinburgh and Glasgow, legitimate births form at least 90 per cent of the whole born. The law therefore regards the ages of the women from whose fertility 90 per cent of the population are recruited.

It must be observed that this law or general statement shows nothing regarding the fecundity of women of different ages, although it has been held as doing so ; it merely enunciates a truth in the doctrine of population. I place it first because it is pretty well known, because in my own investigations it was first made out, and chiefly because it is essential, before proceeding farther, to show the facts on which it is founded in their true light, avoiding the great errors of which similar facts have been made the basis.\*

The facts or data illustrating this law, with which I am best acquainted, have been derived from reports of lying-in dispensaries, as by Dr. Granville, or from

\* See Granville, *Transactions of Obstetrical Society of London*, vol. ii



similar accounts of lying-in hospitals, as by Dr. Collins, Drs. Hardy and M'Clintock, and others. I here present, as an example, the table of ages of mothers of legitimate and illegitimate offspring, whether born alive or dead, from the *Practical Treatise on Midwifery* of Dr. Collins, master of the Dublin Lying-in Hospital. The data adduced by Dr. Granville in the second volume of the *Transactions of the Obstetrical Society of London* are closely similar. Judging from these data, it would appear that most children are born of women at or near the age of thirty years, or the middle of the child-bearing period of life; and that the offspring of mothers of ages advancing from the commencement of child-bearing to the age of thirty, or the middle of the child-bearing period, gradually increases; that the climax is reached at this age, and that thereafter the offspring of mothers advancing above thirty gradually diminishes. But while the age of thirty forms the climax, there is not an equal fertility on either side of it, a much larger part of the population being born of mothers under thirty than of mothers above thirty. Dividing the number of mothers at thirty years, and adding together those on each side of the division, we have on the side of the younger 12,106, and on the side of the elder women 4279, giving a majority of 7827 in favour of the younger; or, otherwise stated, we have three-fourths of the births among the younger half, and only one-fourth among the elder. The mean age of the mothers in Dr. Collins' table is twenty-seven years.



TABLE I.—SHOWING THE AGE OF EACH OF 16,385 WOMEN DELIVERED IN THE DUBLIN LYING-IN HOSPITAL.

Age . . . . .	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32
No. of Women	3	19	70	237	433	926	682	1142	1023	1089	1174	1295	983	1340	517	2346	242	467

Age . . . . .	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	50	53
No. of Women	378	384	396	379	153	217	65	326	15	21	18	17	11	5	6	5	1

TABLE II.—SHOWING THE AGES OF 16,385 WOMEN DELIVERED IN THE DUBLIN LYING-IN HOSPITAL,  
ARRANGED IN PERIODS OF FIVE YEARS.

Age . . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50 and over.
No. of Women . . . . .	762	4862	5309	3817	1210	397	22	6
Percentage . . . . .	4.65	29.67	32.40	23.29	7.38	2.42	.13	.03



The next table which I present shows an arranged collection of data, comprising the wives-mothers of living children born at or near the full time in Edinburgh and Glasgow in 1855. The former table has, regarding the use to be made of it, the advantage over this table, of including all mothers bearing children, whether legitimate or not, alive or dead, in the Dublin Hospital. But in every other respect this second table presents what I judge to be more reliable data. The former table contains a class of cases selected according to complicated conditions which it is impossible to state, but which are the result of the correlated circumstances of the Hospital, and of the class from which in Dublin it draws its patients. In the second table, the conditions of selection are fewer and less important, the chief being the legitimacy, life and maturity, or at least viability, of the offspring. Now the limits of the influence of these different conditions are pretty well known, and the proportional differences between the two tables are too great to be accounted for by these differences. The second table is thus shown to be the more trustworthy.



TABLE III.—SHOWING THE AGE OF EACH OF 16,301 WIVES WHOSE CHILDREN WERE REGISTERED IN  
EDINBURGH AND GLASGOW IN 1855.

Ages .	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
Mothers	4	28	116	228	405	543	828	888	1024	1058	1063	925	1116	875	1214	545	825	645	621

Ages .	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	57	Total
Mothers	691	594	409	426	287	404	142	148	80	66	50	27	6	9	4	—	2	4	1	16,301

TABLE IV.—SHOWING THE AGES OF 16,301 WIVES-MOTHERS IN EDINBURGH AND GLASGOW IN 1855,  
ARRANGED IN PERIODS OF FIVE YEARS.

Ages . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54	55-59	Total
Mothers . . .	376	3688	5037	3850	2407	840	96	6	1	16,301
Percentage .	2.30	22.62	30.89	23.61	14.76	5.15	.58	.03	—	



An inspection of this table shows again that the year of maternal life yielding most recruits to the general population is the thirtieth, and an easy calculation makes out that about three-fifths of the legitimately-born population are derived from women of thirty years and under, while two-fifths are derived from women of thirty years and upwards. For, dividing mothers of thirty years of age, and adding together those on each side of the point of division, we have on the side of the younger 9708 mothers, and on the side of the elder 6593, giving a majority of 3115 in favour of the younger. The mean age of the wives-mothers in this table is above twenty-nine.

From these data I conclude :—

1. That the actual, not the relative, fertility of our female population, as a whole, at different ages, increases from the commencement of the child-bearing period of life, until the age of thirty is reached, and then declines to its extinction with the child-bearing faculty.

2. That the actual fertility is much greater before the climax, thirty years, is reached, than after it is passed.

3. That at least three-fifths of the population are recruited from women not exceeding thirty years of age.

Before leaving these tables, it is expedient to direct attention to a striking lowness of figure at the ages of twenty-nine and thirty-one respectively in Dr. Collins' data. A similar fall on each side of the highest number



occurs in Dr. Granville's table, which has been referred to, and in every similar table which I know. This curiosity has given rise to very natural and ingenious speculation. Dr. Granville suggests that by the earlier decrement nature means to rest awhile, and gather strength for the enormous jump she is to make in the following year, and that by the second decrement she means to evince the exhaustion which invariably follows over-exertion ! But I cannot acquiesce in this fanciful hypothesis, believing that really no such decrement, jump, and second decrement, occur. My explanation of this tabular phenomenon is suggested by the occurrence of similar falls on each side of the age of forty years in Collins' table, and in my own and in others. I am too well aware, from ample experience, of the impossibility of getting women's ages stated correctly, especially if they have passed twenty-five years, and have often observed, that when pushed they say thirty or forty, as a round easy number ; and the state of the tables appears to me merely to indicate that women about thirty-one and forty-one years of age frequently say they are thirty and forty years old, respectively. In short, these decrements are evidence of the unfortunate element of error which creeps into the most carefully-prepared vital statistics on a large scale.



## CHAPTER II.

## THE COMPARATIVE FERTILITY OF THE FEMALE POPULATION AS A WHOLE AT DIFFERENT AGES.

HAVING shown the actual fertility of women at different ages in our population, I now proceed to the question of the comparative productiveness of our whole female population at different ages. To settle this, it is only necessary to compare the number of children born of women of different ages with the number of women living at the different ages respectively. The result of the calculations involved in this comparison will be to show, not simply (as in Chapter I.) the numbers of children born of women at different ages, but the number of mothers relatively to the number of women living at different ages; in other words, the comparative fertility or productiveness of our female population as a whole at different ages.



TABLE V.—SHOWING THE COMPARATIVE FERTILITY AT DIFFERENT AGES OF THE WHOLE FEMALE  
POPULATION OF EDINBURGH AND GLASGOW.

Ages		15-19	20-24	25-29	30-34	35-39	40-44	45-49
Women	. . . . .	31,538	34,631	29,778	24,272	19,362	17,938	13,868
Children	. . . . .	380	3,731	5,096	3,895	2,435	849	97
Proportion of latter to former is 1 in		82·9	9·2	5·8	6·2	7·9	21·1	142·9
Or percentage	. . . . .	1·20	10·77	17·11	16·04	12·57	4·73	·69



Table V. is constructed so as to bring out the desired results, and at the same time be easily compared with Table VI. It must be observed that the fifth table does not exhibit results whose actual amounts are of much value, but results the value of which is very great with a view to determining the question of comparative amounts or productiveness. For, while the numbers of women at different ages include the whole women living at these ages in Edinburgh and Glasgow in 1861, the numbers of children born at different ages, as given in the table, include only children born under the conditions of legitimacy, live birth, and complete or nearly complete maturity in 1855.

In his work on Annuities,\* etc., Mr. Milne in 1815 published a valuable table, which he describes as "showing the fecundity of women at the different periods of life in Sweden and Finland, from 1780 to 1795, both years inclusive." It is taken from a paper by Mr. Nicander, to which he gives a reference, but unfortunately I have not been able to ascertain the exact conditions (if any) under which the table was prepared.

\* Vol. ii. p. 582. Large extracts from the Swedish returns, with remarks, are to be found in the English Registrar-General's Sixth Annual Report, 1844, p. 267, *et seq.*



TABLE VI.—SHOWING THE COMPARATIVE FERTILITY AT DIFFERENT AGES OF THE FEMALE POPULATION  
OF SWEDEN AND FINLAND. (*From the Table of Mr. NICANDER.*)

Ages . . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Females . . . . .	132,765	131,377	121,650	112,250	98,710	89,259	74,002
Deliveries . . . . .	3,298	16,507	26,329	25,618	18,093	8,518	1,694
Proportion of latter to former, 1 in	40·256	7·959	4·620	4·382	5·456	10·479	43·686
Or percentage . . . . .	2·48	12·56	21·64	22·82	18·32	9·54	2·28



In the last line of both Tables V. and VI. it will be remarked that the first and last proportional numbers are very low, or that at the beginning of the scale, at the age of from fifteen to nineteen inclusive, fertility is comparatively very small; and that at the end of the scale, at the age of from forty-five to forty-nine inclusive, fertility is again comparatively very small. This no doubt depends to a great degree on the circumstance, that among the women from fifteen to nineteen years of age are included a large proportion of immature girls, and among the women from forty-five to forty-nine years of age a large proportion of women whose child-bearing powers have disappeared. Keeping in view this undoubted partial explanation of the lowness of the figure, or the lowness of fertility at these ages, the tables are seen to yield interesting results. They show that the fertility of the populations of Edinburgh and Glasgow, and of Sweden and Finland, increases gradually till the middle of the child-bearing period of life, or about the age of thirty years, and that then fertility gradually falls off towards its complete extinction.

My knowledge of the conditions under which my own table was framed, as already stated, being exact, as compared with my knowledge of Mr. Nicander's, I shall, in framing conclusions, adopt the results it affords. On like grounds I shall excuse myself from proceeding to compare the easily-remarked differences of the results of the two tables.

In regard, then, to the comparative fertility of our



whole female population at different ages, I conclude—

1. That it increases gradually from the commencement of the child-bearing period of life until about the age of thirty years is reached, and that then it still more gradually declines.

2. That it is greater in the decade of years following the climax of about thirty years of age than in the decade of years preceding the climax.



## CHAPTER III.

THE COMPARATIVE FECUNDITY OF THE WHOLE WIVES IN  
OUR POPULATION AT DIFFERENT AGES.

I NOW proceed to the question of the fecundity, not fertility or productiveness, of the mass of wives of different ages in Edinburgh and Glasgow. In the two preceding chapters the fecundity or comparative power of production at different ages has not been entered on ; in them have been considered merely the actual production of children by women of different ages, and the comparative amounts of production by the female population at different ages. It is known that at all ages there is a great mass of spinsters whose productiveness is not tested, and it is of course necessary, in order to determine questions of fecundity, to eliminate all women not living in married life, or not having their fecundity tested in the ordinary way, from our observations and calculations. In this chapter, therefore, the comparison is not of mothers with women living, as in Chapter II., but of wives-mothers with wives.



TABLE VII.\*—SHOWING THE COMPARATIVE FECUNDITY AT DIFFERENT AGES OF THE WHOLE WIVES IN EDINBURGH AND GLASGOW IN 1855.

Ages . . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Wives . . . . .	756	8,874	14,622	14,579	11,871	10,506	7537
Wives-Mothers . . . . .	378	3,709	5,065	3,872	2,421	845	96
Proportion of latter to former is 1 in	2.0	2.4	2.9	3.7	4.9	12.4	78.5
Or percentage . . . . .	50.00	41.79	34.64	26.56	20.39	8.04	1.27

\* In this Table the actual numbers are given as nearly as possible.  
The numbers of wives have been arrived at in the following way:—We have the population of Edinburgh and Glasgow in 1851 and 1861, and by calculation estimate the population in 1855. We have the actual numbers of wives of different ages in 1861, and by an easy calculation of proportions we reduce the numbers of wives of different ages to the numbers given for 1855.  
The number of wives-mothers extracted from the registers of 1855 is 16,301, bearing 16,500 legitimate children. But the Registrar-General's reports state the number of children as 16,593. Hence it appears that 93 births are omitted in the extracts. These omissions were made on account of manifest carelessness and inaccuracy in the registers. To these 93 births corresponds the number of 92 mothers, one being deducted for a twin case. These 92 mothers have been added proportionally to the others, in order to make up the total of 16,393.



The seventh table establishes a comparison between the numbers of married women of various ages, and the numbers of such women bearing living children. In Edinburgh and Glasgow, the number of wives within the ages of fifteen and forty-nine inclusive, or who might have borne children in 1855, was 68,745, and the number of wives-mothers in the same population, in the same year, was 16,386,\* or 1 in 4·2. In the tables these are arranged in columns of different ages, so as to exhibit the comparative fecundity of the whole wives of different ages. It will be seen at a glance that the table shows that the fecundity of the mass of wives is greatest in the first years of the child-bearing period of life; and I regret extremely that the data at my disposal do not permit me to condescend on the circumstances in this respect of each individual year. The table shows that, from the earliest years of child-bearing life onwards, the fecundity of the mass of married women gradually wanes to its extinction. It is also easily made out that while there were 24,252 wives under thirty years of age, and of these 9152 bore children, there were 44,493 wives of ages varying from thirty to forty-nine years inclusive, and of these only 7234 bore children; or, to speak in round numbers,

\* The actual number of wives-mothers in Edinburgh and Glasgow in 1855 was 16,393. This figure is in the text reduced to 16,386, and seven wives-mothers omitted, because these seven were altogether exceptional, occurring as they did between the ages of fifty and fifty-seven, and could only damage the statement of results.



the wives under thirty years of age were much more than twice as fecund as the wives above thirty years. But a more interesting and valuable comparison may be made by taking the same number of fifteen years before and after the middle of child-bearing life, a total period of thirty years, which includes the immense majority of child-bearing women. Doing so, we find that of 24,252 wives under thirty years of age, 9152 bore living children, and that of 36,956 wives of ages from thirty to forty-four inclusive, 7138 bore living children. Had the elder women been as prolific as the younger, they would have produced 13,946 children, instead of 7138; that is, the fecundity of the younger women was almost double that of the older.

The data at my disposal enable me to give the figures for each year of age up to twenty. But the numbers are so small that little value can be placed on the results drawn from them. So far as they go, they indicate great fecundity of a mass of wives at seventeen, eighteen, and nineteen.



TABLE VIII.—SHOWING THE COMPARATIVE FECUNDITY OF WIVES AT AGES OF 16, 17, 18, 19, AND 20, IN EDINBURGH AND GLASGOW IN 1855.

Ages . . . . .	16	17	18	19	20
Wives . . . . .	13	55	232	455	1043
Wives-Mothers . . .	4	28	116	228	405
Proportion of latter to } former is 1 in . }	3·25	1·96	2·00	1·99	2·57
Or percentage . . .	30·77	50·91	50·00	50·11	38·83

Table VII. has been prepared so as to give the actual amounts. I found it possible to do this with a near approach to exactness, and it is evident that in this way the results derived are not only comparative statements, with only relative value, but also statements of actual values.

From the data now given I conclude—

1. That the fecundity of the mass of wives in our population is greatest at the commencement of the child-bearing period of life, and after that period gradually diminishes.
2. That the fecundity of the whole wives in our population included within the child-bearing period of life is, before thirty years of age is reached, more than twice as great as it is after that period.



3. That the fecundity of the wives in our population declines with great rapidity after the age of forty is reached.

Some of these conclusions may be stated with the actual numerical results, as follows:—While of all the wives living in Edinburgh and Glasgow between the ages of fifteen and forty-five, one in 3·8, or 26·3 per cent, bore a living child; of those between the ages of fifteen and twenty-nine inclusive, one in 2·6, or 38·4 per cent, bore a living child; and of those between the ages of thirty and forty-four inclusive, one in 5·1, or 19·6 per cent, bore a living child.

It will subsequently be shown that these conclusions regarding a mass of wives are not true, if applied to the individuals forming that mass. A different law governs individuals. Their fecundity is greatest from twenty to twenty-five; that is, a woman marrying at that age is more likely to demonstrate her fecundity than if she married at any other age. But it will naturally be rejoined, if such is true of individuals, why not of masses? In the sequel, the explanation will be given: it is founded on the law of intensity of fertility, and may, for the present purpose, be shortly stated thus:—The comparatively greater intensity of fertility of the fertile wives married from fifteen to twenty, over that of those married at from twenty to twenty-five, does more than make up for the sterility of some of the younger. Though less fecund, they are more fertile as a mass.



## CHAPTER IV.

## THE INITIAL FECUNDITY OF WOMEN AT DIFFERENT AGES.

IN commencing the statistical inquiry whose results I am now giving, my object was to discover the fecundity of women at different ages, and I now proceed to address myself to this point.

It is not my object to illustrate the subject of the arrival of young girls at the age of maturity, the change of the girl into the fertile woman. In the case of some peoples, facts might be collected regarding wives so young as to be in a large proportion sterile from immaturity ; and their fecundity gradually appearing as age advanced, might produce a column of mothers from ten to twenty years of age, showing a gradually-increasing fecundity of the population at these ages. Even in our tables, derived from the data of wives in Edinburgh and Glasgow, some interesting results are to be found, and allowance must be made for a certain amount of immaturity in the wives of from fifteen to twenty years of age. But this question of the arrival of girls at maturity is foreign to the present topic. In it, all the women are supposed to be mature, and subjected to the conditions essential for procreation.



The fecundity of individual women is known to vary extremely. Some are very frequently pregnant, and repeatedly, or even constantly, have plural births, and thrive with it all. Under like conditions other women are absolutely sterile, or a miscarriage or a dead mature child forms the climax of their fecundity; and this little may be effected at the expense of permanent constitutional exhaustion. Between these extremes of great fecundity and absolute sterility there is an unlimited series of varying degrees of fertility. On this interesting aspect of the subject of fecundity the present research throws little light. It is founded on the result of an aggregate of cases, and can show almost nothing as to individuals. It illustrates the fecundity at different ages of women generally, not the individual fecundity of any.

The table given in last chapter (Table VII.), affords data which cannot be applied to settle the question of the fecundity of women of different ages. For it is evident that among the mass of wives of each succeeding year, or series of years, are included the wives who were once of the former series, or part of them—that is, a class of wives whose fecundity has been at least liable to be increased, diminished, or exhausted by procreation, before they have come to form part of the wives in any of the columns after the first. In order to arrive at the fecundity of women or wives at different ages, it is necessary to secure that the conditions of the compared women of these different ages be as nearly the same as possible. This is not attempted in the seventh table.



TABLE IX.\*—SHOWING THE INITIAL FECUNDITY OF WOMEN OF DIFFERENT AGES IN THE FIRST YEAR OF MARRIAGE.

Ages of Wives newly Married . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54	55-59	60-64	65-69	Total.
No. of Wives newly Married . . .	700	1835	1120	402	205	110	46	20	6	2	1	4447
No. of Wives-Mothers within first year of Marriage . . .	96	339	139	46	19	4	...	...	...	...	...	643
Proportion of latter to former is 1 in . . .	7.3	5.4	8.0	8.7	10.7	27.5	...	...	...	...	...	6.9
Or percentage . . .	13.71	18.48	12.41	11.44	9.27	3.63	...	...	...	...	...	14.46

\* The number of wives married at different ages in Edinburgh and Glasgow in 1855 is arrived at in the following manner :—The marriages in Scotland in 1855 were 19,680. The marriages in Edinburgh and Glasgow in 1855 were 4447. The distribution of the 19,680, according to age at marriage, is given at p. 22 of the Registrar-General's Annual Report for 1861. This distribution requires a correction for the number whose ages at marriage were not known. Calculating on the ages of the whole 19,680, the proportional distribution of the 4447 married in Edinburgh and Glasgow is found to be in the table above.



Table IX. is constructed to show the relative initial fecundity of newly-married women of different ages. By the returns of the Registrar-General we calculate how many women at each succeeding year of age contracted marriage in 1855, in Edinburgh and Glasgow. My extracts from the register for 1855 show how many of these women bore living children before they had been a year married. When the two figures are compared for each age, we have the fecundity at the outset of child-bearing at each age. The table reads as follows :—Of 700 women married between fifteen and nineteen years of age inclusive, 96 bore a living child before they had been wives for twelve months, or one in every 7·3 ; and so on.

Table X. is in every respect the same as the former, only it shows the fecundity within twenty-four months of married life ; or the number of women bearing living children in 1855, and before they were two years married, is compared with the number of newly-married. The observation that the fecundity within twenty-four months is much more than twice as much as the fecundity within twelve months after marriage, appears to me to give this table more substantial value than the former, as an indication of the actual fecundity of the outset of child-bearing at different ages.

Both these tables show the highest rate of initial fecundity to be between the ages of twenty and twenty-four inclusive, and a gradual declension from that time on either side as age diminishes or increases.



TABLE X.—SHOWING THE INITIAL FECUNDITY OF WOMEN OF DIFFERENT AGES WITHIN THE FIRST TWO YEARS OF MARRIAGE.

Ages of Wives newly Married . . . . }	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54	55-59	60-64	65-69	Total.
No. of Wives newly Married . . . . }	700	1835	1120	402	205	110	46	20	6	2	1	4447
No. of Wives-Mothers bearing in 1855, and within two years of Marriage . . . . }	306	1661	849	253	84	17	2	...	...	...	...	3172
Proportion of latter to former is 1 in . . . }	2.3	1.1	1.3	1.5	2.4	6.4	23.0	...	...	...	...	1.4
Or percentage . . .	43.71	90.51	75.80	62.93	40.97	15.45	4.35	...	...	...	...	71.33



The two following tables (XI. and XII.) show that on the side of the women younger than twenty years, initial fecundity steadily decreases with age. In regard, however, to these young wives, it may be objected that there is a source of error from immaturity, which is certainly very trifling after the age of twenty is reached. And the objection is, theoretically at least, quite just, for it is absurd to attempt to measure the fecundity of women who have not become sexually mature, and the admixture of immature with mature is a source of error, important, directly according to its amount. It is unsatisfactory merely to allege in answer, that immature girls are not likely to be found among young wives in such numbers as to form a source of great error. I have therefore taken the following means to ensure that this source of error be completely excluded.



TABLE XI.—SHOWING THE INITIAL FECUNDITY OF WOMEN UNDER TWENTY YEARS OF AGE IN  
THE FIRST YEAR OF MARRIAGE.

Ages of Wives newly married	.	.	.	.	.	16	17	18	19
No. of Wives newly Married	.	.	.	.	.	43	108	225	314
No. of Wives-Mothers within first year of Marriage	.	.	.	.	.	2	7	31	56
Proportion of latter to former is 1 in	.	.	.	.	.	21.5	15.4	7.2	5.6
Proportion after correction for Immaturity is 1 in	.	.	.	.	.	15.5	12.8	7.0	5.4
Or percentage	.	.	.	.	.	6.45	7.77	14.70	18.30



TABLE XII.—SHOWING THE INITIAL FECUNDITY OF WOMEN UNDER TWENTY YEARS OF AGE WITHIN THE FIRST TWO YEARS OF MARRIAGE.

Ages of Wives newly Married	.	.	.	.	.	16	17	18	19
No. of Wives newly Married	.	.	.	.	.	43	108	225	314
No of Wives-Mothers within two years of Marriage	.	.	.	.	.	4	27	98	177
Proportion of latter to former is 1 in	.	.	.	.	.	10.7	4.0	2.3	1.8
Proportion after correction for Immaturity is 1 in	.	.	.	.	.	7.7	3.3	2.1	1.7
Or percentage	.	.	.	.	.	12.90	30.00	46.44	57.84



The commencement of menstruation is generally considered by physicians an indication of the arrival of sexual maturity. It may be true that some are still immature in whom this phenomenon has shown itself, and it certainly is true that some are mature before its appearance. Yet it forms a generally accredited indication of maturity.\* The following table (XIII.), framed by Dr. WHITEHEAD, is a large collection of data, showing the age of the appearance of menstruation in 4000 individuals in this country. It is easy to calculate what fraction of the whole 4000 had begun to menstruate at sixteen, seventeen, eighteen, and nineteen years of age respectively ; or, in other words, what fraction was believed capable of exhibiting fecundity at these ages. This I have done, and have corrected the numbers of wives in tables eleventh and twelfth accordingly, reducing them to similar fractional parts. After making this correction for immaturity, I have calculated the proportions of wives-mothers to wives, and placed the results in the last line. They remain the same so far as to show a steady diminution of fecundity as age diminishes.

TABLE XIII.—“SHOWING THE AGE AT WHICH PUBERTY WAS ACCOMPLISHED IN FOUR THOUSAND INDIVIDUALS.”—(WHITEHEAD *on Sterility and Abortion*, p. 46.)

At Age of 10 years		9 first Menstruated.	
„	11	„	26
„	12	„	136

\* See the Discussion on the Age of Nubility. Here, maturity is meant merely to imply ability to bear children, not fitness for married life.



At age of 13 years 332 first menstruated.

„	14	„	638	„
„	15	„	761	„
„	16	„	967	„
„	17	„	499	„
„	18	„	393	„
„	19	„	148	„
„	20	„	71	„
„	21	„	9	„
„	22	„	6	„
„	23	„	2	„
„	24	„	1	„
„	25	„	1	„
„	26	„	1	„

From these data I conclude—

1. That the initial fecundity of women gradually waxes to a climax, and then gradually wanes.

2. That initial fecundity is very high from twenty to thirty-four years of age.

3. That the climax of initial fecundity is probably about the age of twenty-five years.



## CHAPTER V.

## THE FECUNDITY OF WOMEN AT DIFFERENT AGES.

It is plain that many women may prove fecund after two years of married life have passed; and all such women are lost sight of, if we proceed to inquire only in the way adopted in the last chapter, on initial fecundity. The women excluded, by the adoption of the way just named, may be enough to make the laws of initial fecundity deceitful and misleading, if any argument as to fecundity generally be drawn from them.

To make a general table, including all married women, I compare the calculated number of marriages at various ages in Edinburgh and Glasgow in 1855, with the number of first children born in that year of women married at various ages. Although the primiparæ of 1855 will not all be women married in that year, it may be assumed that, if the marriages be nearly alike for some years, the numeration of the primiparæ of one year will give pretty accurately the fecundity of the married women of any year. This process is carried out in the following table:—



TABLE XIV.—SHOWING THE FECUNDITY OF WOMEN MARRIED AT DIFFERENT AGES.

Ages of Wives at Marriage .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50, etc.	Total.
No. of Wives . . . . .	700	1835	1120	402	205	110	46	29	4447
First Children . . . . .	649	1905	809	251	96	10	2	...	3722
Percentage Fertile . . . . .	92·7	100	72·3	62·5	46·8	9·1	4·4	...	83·7
Or 1 in . . . . .	1·08	1·00	1·38	1·60	2·13	11·0	23·0	...	1·19



This table is valuable, in a particular way, confirming the results arrived at in the last chapter, by like results obtained by a different method of analysing the same data. Its results have reference to the average individual woman, not to a mass of women. The conclusions to be derived from it are easily observed:—

1. That nearly all women married at from twenty to twenty-five years of age are fecund.

2. That the fecundity of very young (fifteen to twenty) wives, below twenty, is greater than that of wives married at from twenty-five to twenty-nine.

3. That there is a climax of fecundity in women, which is reached between twenty and twenty-five years of age.

A remarkable illustration of the variation of fecundity at different ages is acquired by observation of the fertility of the domestic fowl.

“It has been ascertained (says Mr. Geyelin)\* that the ovarium of a fowl is composed of 600 ovula or eggs; therefore, a hen during the whole of her life cannot possibly lay more eggs than 600, which in a natural course are distributed over nine years, in the following proportion:—

\* *Poultry-Breeding in a Commercial Point of View.* By Geo. Kennedy Geyelin, C.E. London 1865, p. 18.



TABLE XV.—SHOWING THE FERTILITY OF THE DOMESTIC FOWL AT DIFFERENT AGES.

First year after birth	15 to 20	Sixth year after birth	50 to 60
Second „	100 „ 120	Seventh „	35 „ 40
Third „	120 „ 135	Eighth „	15 „ 20
Fourth „	100 „ 115	Ninth „	1 „ 10
Fifth „	60 „ 80		

“ It follows that it would not be profitable to keep hens after their fourth year, as their produce would not pay for their keep, except when they are of a valuable or scarce breed.”

At this point my present inquiry is closed. I know of no other way of advancing our knowledge of this subject than by the collection and analysis of statistics. The only quarry for such materials, that I know of, is the Scottish registers for 1855. The tables adduced might be improved by going over a larger field, and increasing the numbers analysed. But I do not see how the matter in the registers could be turned to more account without encroaching on another topic which is at the same time closely connected with that under discussion—viz. the fertility of marriage. Or, as marriage is scarcely admissible as a term in physiology, I should give this subject the title of “sustained fecundity,” the degrees of fertility which women of different ages, beginning to live with men, continue to exhibit during the child-bearing period of life.

The views hitherto entertained regarding the influence of age on fecundity have been various. “In regard to age (says Burdach\*) fecundity is diminished

\* *Physiologie*, tom. ii. p. 117.



in the first and last portions of the continuance of the aptitude for procreation. The elk, the bear, etc., have at first only a single young one, then they come to have most frequently two, and at last again only one. The young hamster produces only from 3 to 6 young ones, whilst that of a more advanced age produces from 8 to 16. The same is true of the pig. This rule appears to be general, since it applies also to the Entomostracæ; according to Jurine, the number of the young of the *Monoculus pulex* is at first from 4 to 5, afterwards rising gradually as high as 18. We scarcely ever encounter the births of 3 or 4 children except in women who have passed the thirtieth year. Precocious marriages are not only less fertile, but the children also which are the result of them have an increased rate of mortality. According to Sadler, every marriage in the families of the peers of England yields 4·40 children when the woman was married below sixteen years of age; 4·63 from this age to twenty; 5·21 from twenty to twenty-three; and 5·43 from twenty-four to twenty-seven." The notions here expressed by Burdach are in the main correct; but it is evident that they are very indefinite. They are to be regarded, also, more in the light of happy guesses than of well-founded opinions. Burdach evidently places chief reliance on the evidence afforded by the numbers at a birth. From many quarters I have received corroboration of Burdach's statements regarding the increase and subsequent decrease of the number produced at a birth by pluriparous animals, and I have received



similar information regarding bitches, guinea-pigs, etc. When I first paid attention to this subject, the plural births of women appeared to me to form a simple key for the determination of the fecundity of women at different ages. But I soon became dissatisfied with the materials I quickly collected. Woman is not a pluriparous animal, neither does she produce so regularly, or according to season, as the animals with which she is compared. In her the occurrence of twins and triplets is an exception to the normal rule, and the number of children born by her cannot be so simple and sure a test of fecundity as in the case of animals having multiple litters at stated periods. Indeed, it is apparent that the evidence derived from plural births alone in women may positively mislead, for a woman may be more fertile bearing one child at a time frequently than another bearing twins or triplets more seldom. In this place I shall only say that the numerical study of twins, in reference to the age of the mother, yields interesting results, which do not confirm Burdach's statement regarding them, yet are not hostile to the conclusions here stated. Burdach, in his work, describes an annual rise and fall in the fecundity of some pluriparous animals. This annual variation forms a series of wavelets in the course of the great wave running from youth to old age, and culminating in middle life. This annual rise and fall of fecundity he attributes to the influence of cold.\*

\* This influence of cold has always been a favourite notion. It has some poetical truth, but it has never, so far as I know, got any



In his *Treatise on Man*, M. Quetelet has, with some care, collected the statistical materials available at the time for advancing the settlement of the question of the relation of age to fecundity. He does not allude

substantial basis. Roberton's labours on the influence of heat and cold upon the commencement of menstruation have ended chiefly in negative results, and have tended rather to overthrow than confirm what may be called the popular opinions. They are to be found in his work on *Physiology and Diseases of Women, and on Midwifery*.

More recently Dr. Stark, in an official report to the registrar-general for Scotland, has, by means of a comparison of the Swedish returns with those of Scotland, arrived at a definite conclusion regarding the influence of cold. "The comparison," he says, "would seem to indicate that cold does delay the childbearing period of life in women, as has been often supposed, but never till now proved by trustworthy facts" (*Eighth detailed Annual Report*, 1866, p. xv.) Speaking of Sweden, he remarks that—"it would appear that the childbearing period of life in that country is, as it were, delayed for a period of about four years later than in this country. Thus, instead of the greatest number of women bearing children between the ages of twenty-five to thirty, as in this country, in Sweden the greatest number bear children between the ages of thirty to thirty-five years. Then, instead of the next most prolific age being, as it is in Scotland, from twenty to twenty-five, and from thirty to thirty-five years of age, in Sweden it is from twenty-five to thirty, and from thirty-five to forty years of age, while nearly the same number of women bear children above forty as between twenty and twenty-five years of age. These facts are so singular, that in Table VIII. are given the number and proportion of mothers at different ages in Scotland and in Sweden, that the difference in the prolific periods of the females of each country may be seen at a glance. By comparison of the facts, it will be observed that, at all ages under twenty-five years, only half the number of women bear children in Sweden as compared with Scotland; whereas, at the other end of the childbearing period, only half the number of



to the opinions of Burdach, probably because they have no sufficient foundation, but he refers to Milne, Malthus, Sadler, Granville, Finlayson, and several foreign authors, who have more or less directly tried to throw light on the topic. Quetelet's whole chapter

women above forty years of age bear children in Scotland, as in Sweden."

These striking facts demand the study of physiologists if they have the bearing upon the influence of cold which Dr. Stark ascribes to them. But we cannot, in the present state of our knowledge, admit the validity of this bearing. We hesitate before accepting the doctrine of Dr. Stark regarding cold. The differences between the two countries may be explained by cold, but we do not think this probable, for they appear to us far too considerable at all ages, and specially too great at the late ages (above forty) to be accounted for by the influence of cold. Besides, in order that such tables as Dr. Stark adduces, be admitted as proving anything, it is necessary first of all to collate with them a table of the ages of Swedish women at marriage for comparison with similar Scottish tables. Difference in the ages at marriage may account for all the striking facts in the table. Indeed, the observation has repeatedly forced itself upon me, that comparisons of this kind made between two countries are dangerous foundations for argument, so great is the risk of error from the conditions of the statistics differing in the countries. [See foot-note, p. 107.]

Whatever value may be ascribed to the opinion of Burdach and Stark as to the influence of cold, or to the statement of Burdach, unsupported as it is by data, that there is an annual rise and fall in the fecundity of some pluriparous animals, I think the observation of the size of a yearly series of hen's eggs lends some shadow of confirmation to the supposed existence of an annual rise and decline of fecundity. For I am informed by more than one hen-wife that the first and last of a hen's yearly series of eggs are smaller than the eggs laid when the process is in undisturbed operation. But I have found no satisfactory ground of connection of this phenomenon with degrees of temperature.



on the influence of age on the fecundity of marriages is very unsatisfactory. It is at least difficult to reconcile with one another the conclusions arrived at in various parts of this chapter, and I shall not attempt to do it. It is only fair to say that he seems conscious of the numerical deficiency of data sufficient for a basis of any conclusion, and as an example of the state of matters, the table of Sadler, which he and Burdach both quote, may be mentioned; in it, the number of marriages analysed is under 500, and they are all selected according to extraordinary conditions. The final conclusion which M. Quetelet announces, is, that it is before the age of twenty-six years that we observe the greatest fecundity in women.

The latest writer on this topic, whom I know of, is Dr. Granville, who, in an interesting paper in the London Obstetrical Transactions, returns to the description of his former labours in the same field. In this paper, production or fertility is confounded with productive power or fecundity, and the table to which I have alluded in Chapter I. he describes not as showing the fertility at different ages of the industrial classes of the metropolis, but erroneously, as showing the alternations in the productive power of women at different ages.

In this part, then, I have, *inter alia*, shown that the great majority of the population is recruited from women under thirty years of age; but that the mass of women in the population, of from thirty to forty years of age, contribute to the general fertility a larger pro-



portional share than the mass of women of from twenty to thirty years of age :—

Further, that the wives in our population, taken collectively as a mass, show a gradually decreasing fecundity as age advances; but that the average individual wife shows a degree of fecundity which increases till probably about the age of twenty-five, and then diminishes.

The fecundity of the average individual woman may be described as forming a wave which, from sterility, rises gradually to its highest, and then, more gradually, falls again to sterility.







## PART II.

### ON THE WEIGHT AND LENGTH OF THE NEWLY-BORN CHILD.

INQUIRING into the influence of the age of the mother upon fecundity, I desired to find out if any light could be thrown upon the subject by the variations, if any, of the weight and length of mature children born of women of different ages; intending to assume that the weight and length of the child might increase or diminish with the high or low state of the fecundity of woman, or of the vigour of the generative functions.

The observations, upon which all my conclusions are founded, have been drawn from the records of the Edinburgh Royal Maternity Hospital. They amount to 2070 pregnancies, with 2087 children. They are not nearly so numerous as I could desire, but no more are available to me.



## CHAPTER I.

ON THE INFLUENCE OF PRIMOGENITURE ON THE WEIGHT  
OF THE NEWLY-BORN CHILD.

PROFESSOR HECKER of Munich, in a recent work,\* publishes some calculations made to show the mean weight of the children of primiparæ as compared with those of multiparæ, and he finds that the latter exceed the former in weight. His figures are as follows :—378 children of primiparæ weighed, on an average, 7·07 lbs. each ; 718 children of multiparæ weighed, on an average, 7·38 lbs. each ; among these 1096 infants, the average weight of the mature children of primiparæ was less than that of the mature children of multiparæ by ·309 lb.

My observations on 2053 children confirm those of M. Hecker. The average weight of 1011 children of primiparous females was 7·170 lbs. ; the average weight of 1042 children of multiparous females was 7·277 lbs. ; the average weight of mature children of primiparæ being less than that of the mature children of multiparæ by ·107 lb.

\* *Klinik der Geburtskunde.* Von Dr. C. Hecker und Dr. L. Buhl. S. 46. Leipzig 1861.



If these results are subjected to some study, their apparent value almost entirely disappears. Hecker evidently would imply that primogeniture is the circumstance which determines the comparative lightness of first-born children, and *vice versâ*. But it is evident that, in order to a just comparison of the weights of children of primiparæ with those of multiparæ, the children compared must be born in circumstances as nearly identical as possible. Especially, care must be had that the now known influence of age of the mother be taken into account, and this care M. Hecker has altogether omitted, an omission for which he is not in any degree blameable, seeing that, when he wrote, the influence of age was not discovered.

In the following investigation it will, I think, be established that some connection exists between variations in the weight of the newly-born child, and, not primiparity or multiparity, but the age of the mother at the time of the birth. No doubt, any statistic of a population or of an hospital may show greater weight in second and subsequent births than in first, because the great majority of primiparæ are young, and their age, anticipating the arrival of the climax of fecundity, may tell upon the size of their offspring. The following considerations seem to me almost to prove the nullity of influence on weight exercised by primiparity.

1. The weight of the children of primiparæ is not nearly uniform, but varies according to the law of the age of the mother. (See Table XX.)

2. The weight of the children of all mothers,



whether primiparæ or multiparæ, varies according to the same law of mother's age. (See Table XVII.)

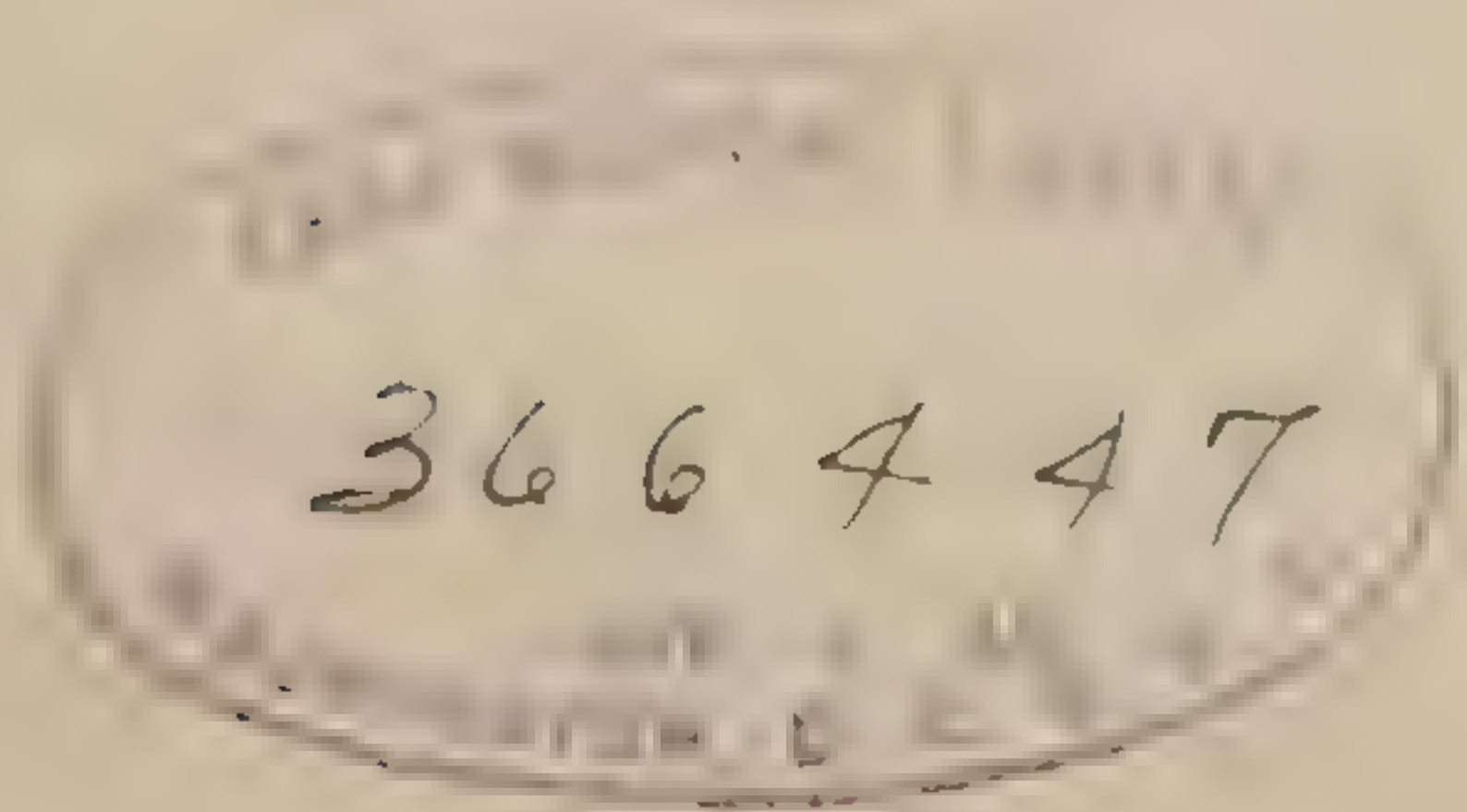
3. The following Table (XVI.) of the mean weights of the children of first and subsequent pregnancies shows no increase or decrease according to the number of the pregnancy. No doubt the children of first pregnancies are lightest, but this may be accounted for by age. After the first pregnancy there is no variation according to any law. There is, indeed, great uniformity in pregnancies after the first. I have appended a line of mean ages to show that even the average woman of seventh and subsequent pregnancies has not passed into the ages in which the decline of fecundity is strongly marked. It is perhaps on this account that no gradual diminution of weight is observed in the more advanced pregnancies. I may add that I have an impression that, were data forthcoming to extend this table to eighth, ninth, tenth, eleventh, twelfth pregnancies, and so on, a diminution of the mean weight of the children would appear, and that it would correspond to the average age of the woman, advancing in these pregnancies into years of decided decline of fecundity.



TABLE XVI.—SHOWING THE AVERAGE WEIGHT OF CHILDREN BORN IN FIRST AND SUBSEQUENT PREGNANCIES.

No. of Preg nancy .	1	2	3	4	5	6	7, etc.	Total.
Weight of Child .	lbs. oz. 7 3·157	lbs. oz. 7 4·897	lbs. oz. 7 5·597	lbs. oz. 7 3·046	lbs. oz. 7 7·223	lbs. oz. 7 5·076	lbs. oz. 7 4·991	lbs. oz. 7 4·109
Age of Mother . .	22·787	25·806	27·701	30·321	30·424	32·045	35·562	25·625

E





Before advancing, I may here interpolate a remark regarding the difficulty and danger of labour in primiparæ. The popular notion of the increased difficulty and danger of a first labour in a woman no longer young may find its explanation, in part at least, in the increased bulk of the child as indicated by its increased weight and length in such women. I have often heard that an old primiparous woman passes through labour more easily than one somewhat younger, and we may possibly find this also accounted for by the comparatively small size of children born in the latest periods of fecundity.



## CHAPTER II.

## THE VARIATION OF THE WEIGHT OF THE NEWLY-BORN CHILD ACCORDING TO THE AGE OF THE MOTHER.

I HAVE already shown that the fecundity of the average individual woman increases to about the age of twenty-five years. This conclusion receives very interesting corroboration from the following Table (XVII.) of the average weights of mature children born of mothers of ages gradually rising to twenty-five years. The weight of the child gradually increases to its climax in the age of from twenty-five years to twenty-nine. After this age the weight of the child declines, and the diminution goes on by very slow degrees. This slowness is entirely out of proportion with the rapid diminution of fecundity of women of similar ages. But, in the present deficiency of data, it will be well not to attempt to push the discussion farther.



TABLE XVII.—SHOWING THE AVERAGE WEIGHT OF CHILDREN BORN OF MOTHERS OF DIFFERENT AGES.

Age of Mother.	Number of Pregnancies.	Total Weight.		Average Weight.	
		lbs.	oz.	lbs.	oz.
15—19	209	1459	10	6	15·741
20—24	832	6008	7½	7	3·547
25—29	570	4220	4	7	6·463
30—34	278	2020	6	7	4·281
35—39	139	1011	2	7	4·388
40—44	38	272	0	7	2·526
45—49	3	20	12	6	14·666

The decrease of children's weight may perhaps not be justly compared with the decrease of fecundity, for this last decrease is produced chiefly by women entirely deserting the category of the fecund, and bearing no children for comparison. The decrease of the fecundity of the average woman is not produced by diminished fecundity of the individual woman, but by the arrestment or disappearance of fecundity in women previously fertile.



## CHAPTER III.

ON THE INFLUENCE OF PRIMOGENITURE ON THE LENGTH  
OF THE NEWLY-BORN CHILD.

It will be interesting now to inquire if Hecker's opinion regarding the influence of primogeniture be confirmed by a study of the varying lengths of children. Hecker himself has, in his clinical work, made no observations on this point, and it must, I think, be admitted that length of mature infants is not nearly so good a test of growth and nourishment as weight. Yet it will immediately appear that interesting corroboration of other allied laws, if not of Hecker's, may be drawn from a study of lengths; and it was not to be expected that the estimation of such measurements should be omitted by an obstetrician who has laid great stress on the value of length as an indication of maturity, enunciating the doctrine that good evidence of maturity cannot be obtained so satisfactorily by weighing as by measuring.

Among 2053 mature single children there were 1011 born of primiparæ; the average length of these was 19·213 inches;—there were 1042 born of multiparæ; the average length of these was 19·202 inches; the average length of mature children of primiparæ



exceeding that of the mature children of multiparæ by ·011 inch. The difference here in favour of primiparæ is so slight that it may be disregarded. No notable difference in this respect, therefore, is made out between primiparæ and multiparæ, a circumstance which shows that weights and lengths of children have no direct or certain relation, one to another, if the figures adduced have been carefully ascertained and are numerous enough to justify such a statistical conclusion.

The remarks, which I have already made in connection with the corresponding observations on the weight of children of primiparæ and multiparæ, are so closely applicable, *mutatis mutandis*, to the present topic also, that I shall not repeat them. I shall only here give a table of the lengths of children born in first and succeeding pregnancies, to show that there is no increase or decrease of length according to the number of the pregnancy; that length does not seem to be under any law connected with the first or subsequent occurrence of pregnancy.



TABLE XVIII.—SHOWING THE AVERAGE LENGTH OF CHILDREN BORN IN FIRST AND  
SUBSEQUENT PREGNANCIES.

No. of Pregnancy .	1	2	3	4	5	6	7, etc.	Total.
Length of Child . .	Inches. 19·197	Inches. 19·239	Inches. 19·304	Inches. 18·959	Inches. 19·273	Inches. 18·962	Inches. 18·991	Inches. 19·188
Age of Mother . . .	22·787	25·806	27·701	30·321	30·424	32·045	35·562	25·625



Although, however, the facts here adduced do not show the influence of age in conjunction with primiparity, I have little doubt that a large enough collection would show comparative shortness of firstborn children, just as comparative lightness has been shown, and for the same reason—namely, because primiparous women are, in a very large proportion, young.



CHAPTER IV.

THE VARIATION OF THE LENGTH OF THE NEWLY-BORN CHILD ACCORDING TO THE AGE OF THE MOTHER.

Although the observations I have tabulated bear no evidence in regard to the influence of primiparity or of multiparity, yet when thrown together so as to be questioned regarding their relation to maternal age, they support the doctrine which I have elsewhere maintained. Length of the newly-born child is shown in Table XIX. to increase as the mother gets older until the period from 25 to 29 is reached: after this, the length of the child gradually diminishes.

TABLE XIX.—SHOWING THE AVERAGE LENGTH OF CHILDREN BORN OF MOTHERS OF DIFFERENT AGES.

Age of Mother.	No. of Children.	Total Length.	Average Length.
		Inches.	Inches.
15—19	209	3,972½	19·007
20—24	839	16,082¼	19·168
25—29	574	11,109½	19·355
30—34	280	5,384¼	19·229
35—39	142	2,683¾	18·899
40—44	39	737½	18·910
45—49	3	54½	18·166



It appears, then, that a careful study of the weights and lengths of newly-born mature children lends some support to the doctrine that the vigour of the female reproductive system waxes till the age of about 25 years is reached, and then wanes.

I here append two general tables containing numerous details which speak for themselves :—



TABLE XX.—SHOWING THE AVERAGE WEIGHT OF CHILDREN BORN AT VARIOUS AGES AND PREGNANCIES OF MOTHERS.

No. of Preg-nancies.	Age of Mothers.	Pregnancy 1.		Pregnancy 2.		Pregnancy 3.		Pregnancy 4.		Pregnancy 5.		Pregnancy 6.		Pregnancy 7, etc.		Total.	
		lbs.	oz.	lbs.	oz.	lbs.	oz.	lbs.	oz.	lbs.	oz.	lbs.	oz.	lbs.	oz.	lbs.	oz.
209	15-19	6	15·772	6	10·733	11	5·000	...	...	6	15·273	...	...	...	...	6	15·741
832	20-24	7	3·455	7	4·486	6	13·735	7	12·273	6	15·273	8	4·000	5	8·000	7	3·547
570	25-29	7	4·836	7	7·481	7	8·451	7	1·447	7	13·565	7	9·400	7	2·125	7	6·463
278	30-34	7	5·283	7	1·733	7	2·971	7	1·864	7	6·097	7	3·520	7	10·270	7	4·281
139	35-39	7	0·846	7	5·846	7	13·444	6	15·364	7	5·167	7	2·765	7	4·289	7	4·388
38	40-44	8	8·000	6	7·200	7	12·000	7	10·750	7	14·000	6	13·333	6	15·000	7	2·526
3	45-49	...	...	...	...	6	0·000	...	...	...	...	...	...	7	6·000	6	14·666
1	50	...	...	...	...	...	...	8	8·000	...	...	...	...	...	...	8	8·000
Average Total		7	3·157	7	4·897	7	5·597	7	3·046	7	7·223	7	5·076	7	4·991	7	4·109
{ Mean Age of Mother		22·787 Years.		25·806 Years.		27·701 Years.		30·321 Years.		30·424 Years.		32·045 Years.		35·562 Years.		25·625 Years.	



TABLE XXI.—SHOWING THE AVERAGE LENGTH OF CHILDREN BORN AT VARIOUS AGES AND PREGNANCIES OF MOTHERS.

Number of Children.	Age of Mothers.	Pregnancy 1.	Pregnancy 2.	Pregnancy 3.	Pregnancy 4.	Pregnancy 5.	Pregnancy 6.	Pregnancy 7, etc.	Total.
		Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
209	15-19	18.992	18.966	22.500	19.682	19.636	18.000	18.000	19.007
839	20-24	19.142	19.171	19.321	18.697	19.435	19.225	19.687	19.168
574	25-29	19.399	19.382	19.455	19.125	19.218	18.640	19.487	19.355
280	30-34	19.739	19.250	18.819	18.250	19.028	18.941	18.685	19.229
142	35-39	19.115	19.086	19.600	19.250	18.500	20.333	18.605	18.899
39	40-44	19.750	18.500	18.750	19.250	18.500	20.333	18.605	18.910
3	45-49	...	...	18.500	...	...	...	18.000	18.166
1	50	...	...	...	21.000	...	...	...	21.000
Average Totals.		19.197	19.239	19.304	18.959	19.273	18.962	18.991	19.188
{ Mean Age of Mother		22.787 Years.	25.806 Years.	27.701 Years.	30.321 Years.	30.424 Years.	32.045 Years.	35.562 Years.	25.625 Years. }



## CHAPTER V.

## PROFESSOR HECKER'S OBSERVATIONS.

MY inquiries were first published in the *Edinburgh Medical Journal*, December 1864. That article called forth from Professor Hecker of Munich a very complete and elaborate article on the same topic. It appeared in the *Monatsschrift für Geburtskunde und Frauenkrankheiten*, November 1865. The Bavarian professor's results are not identical with mine, though they tally with them in one of the chief conclusions. While mine are founded on 2087 observations, Hecker's have for a basis the far larger number of 4449, and on this account alone demand a higher degree of confidence. But I have preferred to adhere in the meantime to the form of my own original statements, and to add to them here the conclusions of Hecker. For neither Hecker's nor my numbers show a satisfactory or assuring amount of steadiness in the increase and decrease of the figures ; and, considering the small differences which the figures show as they increase or decrease, it is necessary, in order to have considerable assurance as to the reliability of the results, to possess a much larger statistical basis than even the 4449 observations of Hecker. I have already said that Hecker's numbers give his conclusions a value superior to that of mine,



and I must add that I hold Hecker to have demonstrated an increase of weight and length of the child with the number of the pregnancy. Only it appears to me that this conclusion of Hecker must be applied, not generally to all pregnancies, but only as far as the fifth ; for his figures are given only for the pregnancies numbering from first to fifth.

I now give the conclusions of this author—

“1. The view of Matthews Duncan, that increase of weight and length of the child is in direct dependence on the age of the mother has been shown to be quite correct.

“2. On the other hand, it is not established that there is, within the child-bearing period of woman's life, a distinct climax of increase of its weight and length ; it is rather shown that increase, on the whole, goes on till the end of fruitfulness.

“3. Age is not, as Duncan implies, the only factor of this increase, but an influence must be distinctly recognised as exerted by the number of the pregnancy.”

The results of Hecker are not without support in the general history of fruitfulness of women, as will be shown hereafter. They may, if verified, stand beside the law of the intensity of fertility of women as age at marriage advances, or as families increase in number. On the other hand, if my conclusions come to be confirmed, they may take their place with the law of the rise and decline of the fecundity of women.\*

\* A study of the domestic fowl's eggs seems to lend some confirmation to my statistical results. The small eggs of the young



In leaving this topic, I wish to point out another subject for investigation, from whose elucidation some explanation of the variations of the weight and length of the newly-born child may arise. Hecker, Clay, Montgomery, and Joulin mention the comparative shortness of the pregnancy of primiparæ. This may account for the smallness of first-born children; and a similar relation may be established between the duration of pregnancies of different numbers, and of women of different ages, and the relative weight and length of the resulting offspring.

Dr. Montgomery's\* opinions that there is no good foundation for any such rule as would affirm that the duration of human gestation is directly proportioned to the age of the woman, and that there is no relation between the duration of pregnancy and the size of the child produced, need not discourage the inquirer, for they are founded, as a perusal of his work shows, more upon individual instances than upon a large collection of data. I shall refer to the latter of these opinions when I discuss the protraction of pregnancy in the latter part of this volume. Dr. Clay† has, in a formal

hen are generally known. I am informed by two experienced henwives that the old hen also lays a comparatively small egg. An observation of my own shows that these eggs are sometimes entirely without yolk, or with an imperfect one.

\* See Montgomery, *Signs and Symptoms of Pregnancy*, 2d ed. p. 535, etc.

† *Observations on the Term of Utero-Gestation*. An interesting essay by Charles Clay, M.D. London, 1855.



manner, and in direct opposition to M. Tessier,\* laid down the proposition, that, as age increases, the term of utero-gestation is lengthened ; and, so far as they go, his observations confirm it. But they are too few for a satisfactory basis, and they require to be corrected for the influence of the number of the pregnancy. The probability that age might exercise an influence on this term was long ago indicated by Condorcet.†

\* *Mem. de l'Acad. Royale des Sciences*, 1817, p. 16.

† *Ibid.* p. 3.



## PART III.

### ON SOME LAWS OF THE PRODUCTION OF TWINS.

HITHERTO the variations of fecundity have been most frequently and most easily observed in animals which have ordinarily multiple births, and the number of whose young, born at different litters, varies. Such difference in number at different times cannot fail to strike the observant eye, and little ingenuity is needed to make the number of young a test of the degree of fecundity. To such an argument I can find no objection.

Among women, the birth of twins occurs once in about eighty deliveries. Triplets, quadruplets, quintuplets, and even higher figures, are occasionally observed; they are very uncommon, and the rarity is progressive with the number. The normal or ordinary rule in woman is to bear one child at a time; and the next most frequent condition is temporary or persistent sterility—two points in which she signally differs from what is generally believed of the animals subjected to the observations referred to.

It is easy to understand how a regularly-bearing animal, the number of whose young born at different litters varies, may in its individual history give good



illustration, if not evidence, of some law of the variation of fecundity. In irregularly-bearing uniparous woman no such illustration or evidence of any law can be got by observing an individual, and recourse must be had to statistics, to the analysis of the details regarding the pregnancies of large numbers of women, subjected to the conditions essential to fecundity.

The rarity of a plural birth in woman, and the increased danger to both mother and offspring in these circumstances, render such an event, in a certain limited sense, an abnormity.\* This abnormity, if used alone, is not a good test of fecundity, and it has been resorted to for this purpose in consequence of the

\* On this subject, consult a valuable paper by Dr. Arthur Mitchell, published in the *Medical Times and Gazette* of November 15, 1862. In that paper Dr. Mitchell demonstrates the abnormality of twin-bearing in the human female ; and to his other proofs he might have added the occurrence of acephalous monsters, only as one of twins. His conclusions are :—

“ 1. Among imbeciles and idiots a much larger proportion is actually found to be twin-born than among the general community.

“ 2. Among the relatives of imbeciles and idiots twinning is also found to be very frequent.

“ 3. In families, when twinning is frequent, bodily deformities (of defect and of excess) likewise occur with frequency.

“ 4. The whole history of twin births is exceptional, indicates imperfect development and feeble organisation in the product, and leads us to regard twinning in the human species as a departure from the physiological rule, and therefore injurious to all concerned.

“ 5. When we pass from twins to triplets and quadruplets, everything we know regarding these latter gives support to the general conclusions in question.”



enticement of its apparently simple and easy application to woman as to the lower animals.

It is, at first sight, evident that a woman, even repeatedly bearing twins, may be surpassed in fecundity by another bearing single children more rapidly or for a longer period. Neglect of these and other such truths has led to errors; and I may cite a popular hasty conclusion regarding the comparative fertility of races in illustration. The greater frequency of plural births in one race than in others, as in the Irish compared with the English, has been represented as showing that the former is more prolific than the latter. Whether the conclusion be true or not, I do not here propose to inquire;\* certainly, the grounds of the conclusion are insufficient to establish it, and unsatisfactory, seeing that an opposite conclusion is not absolutely inconsistent with them, as I have just shown. And a little ingenuity easily discovers other arguments

\* The facts as they appear in Collins' data, and in my statistics of Edinburgh and Glasgow in 1855, are as follows:—In the Dublin Lying-in Hospital 16,385 women produced 480 twin children. In Edinburgh and Glasgow in 1855, 16,301 wives produced 396 twin children. As already often repeated, the two sets of cases are not suited for exact comparison. The result (including all sources of error) is, that the women delivered in Edinburgh and Glasgow should have produced 477 twin children instead of 396, if they were not to allow themselves to be surpassed in double births. The variation of the frequency of twin births in different countries is so great (Oesterlen, *Handbuch der medicinischen Statistik*) as to remove all probability from the notion or belief that the greater or less frequency of twins shows greater or less general fertility. See also Boudin, *Geographie et Statistique Medicales*, tome ii. p. 62.



against the popular view; for example, it might be said that twins, as an unnatural and excessive strain upon the female reproductive powers, may exhaust the fecund energies, and lead to comparative barrenness subsequently.

In illustration of the treatment of this subject by physiologists, I cite the following passage from the works of Burdach :\*—"In regard to age (says he), fecundity is diminished in the first and last portions of the continuance of the aptitude for procreation. The elk, the bear, etc., have at first only a single young one, then they come to have most frequently two, and at last again only one. The young hamster produces only from three to six young ones, whilst that of a more advanced age produces from eight to sixteen. The same is true of the pig. This rule appears to be general, since it applies also to the Entomostracæ; according to Jurine, the number of the young of the *Monoculus pulex* is at first from four to five, afterwards rising gradually as high as eighteen. We scarcely ever encounter the births of three or four children except in women who have passed the thirtieth year.†

\* *Physiologie*, tome ii. p. 117.

† There is a general deficiency of evidence for Burdach's statements. In order to test his assertion concerning the ages of women bearing triplets and quadruplets, I have hastily collected the following ten authentic cases of triplets from the works of Collins, M'Clintock and Hardy, Braun Chiari and Spæth, Hugenberg, and from my note-book. They speak for themselves, and it will be observed that triplets are by no means exclusively confined to women above thirty years of age. Yet it is noticeable that not one



Precocious marriages are not only less fertile, but the children also which are the result of them have an increased rate of mortality. According to Sadler, every marriage in the families of the peers of England yield 4·40 children when the woman was married below sixteen years of age ; 4·63 from this age to twenty, 5·21 from twenty to twenty-three, and 5·43 from twenty-four to twenty-seven.”

In support of Burdach’s statements generally, I here quote an extract from a letter on this subject from the highly intelligent gamekeeper of a Scottish nobleman. It forms a very favourable example of the kind of information I have been able to collect on this topic. “My observation has led me to believe that, as a general rule, a bitch has fewer puppies the first, and gradually increases year by year till her strength begins to fail. Then a rapid diminution follows, ending in one or two. This rule, however, is very materially affected by circumstances, and one may

occurs among the younger child-bearing women, and not one in a first pregnancy.

TABLE XXII.—TABLE OF TEN CASES OF TRIPLETS.

Age of Mother . . .	27	27	29	30	30	33	35	35	36	37
No. of Pregnancy. .	3	3	2	3	11	2	3	6	3	2

An interesting fact in connection with this subject is mentioned in Hugenberg’s report of the St. Petersburg Midwives’ Institute (1863). Three women admitted there between 1845-59 in their fifteenth pregnancies had triplets, and each had triplets three times in succession.



come to very erroneous conclusions by overlooking these. Such, for instance, is the state of the health, condition, amount of work. According to the state of the health, there may be many or few (puppies), weak or strong production ; according to the condition, there will be few or many. If the animal is in a good fair condition, there will be many ; if overfed, few. And as regards the amount of work a dog has to perform, so will the decline be rapid or gradual ; and hence, if a bitch is worked hard year after year, she will fail rapidly, and the diminution of her puppies will be accordingly ; but if worked moderately, and well kept, she will fail gradually, and the diminution will be less rapid.

“ The above rule holds good in reference to breeding sows, but it is a common thing in their case for condition (fatness) to interrupt the rule. When a sow or bitch in an overfed state is brought in contact with the male, and afterwards increases in fat, which in this state they are very liable to do rapidly, consequently there must be a pressure upon the womb, and hence the embryo will be crushed, and thus production will be prevented. I believe it is well known how easily the embryo can be injured or destroyed, but I do not think it is well known how much it will endure in the foetal or more advanced state. We had an instance of this last year, when the great spate was in the month of May, when so many of our pheasant eggs were carried away by the flood and covered in water, many of which we lifted from under one and two feet of water,



and many we got floating about in the ditches, which had been a night and a good part of a day in the cold water. Now, we knew that many of these eggs had been a good while sat upon; and being anxious if possible to save some of them, got them set under common hens, and many of them hatched wonderfully well: some of them came out a few days after we set them. Now, had these eggs been in the embryo state a little shake and being in cold water an hour or two would have rendered them quite useless.

“I have never observed any cause that led to a greater proportion of males or females in a litter. From what I have observed, I do not think that age or strength on either side has anything to do with it.”



## CHAPTER I.

THE NUMBER OF TWINS BORN OF WOMEN OF  
DIFFERENT AGES.

I HAVE made a collection of 1512 twins from the sources indicated in the footnote,\* and have arranged them so as to show the various ages of their mothers in quinquennial periods. This table shows that in the general population it so happens that the number of twins born increases with the age of the mother until the age from twenty-five to twenty-nine inclusive is reached, and that after this age is passed the number of twins born regularly diminishes,—a result which accords with what is observed of the fertility generally of the whole female population; the actual fertility of the female population increasing from the commencement of the child-bearing period of life until the age of thirty is reached, and then declining to its extinction with the child-bearing faculty.†

\* Collins, *Practical Treatise*; private letter from Dr. M'Clinck, giving extracts from the Dublin Hospital Register; M'Clinck and Hardy on *Midwifery and Puerperal Diseases*; Chiari Braun und Spæth, *Klinik der Geburtskunde*; my own extracts from the Registers of Edinburgh and Glasgow for 1855.

† *Transactions of the Royal Society of Edinburgh* for 1863-64, p. 479; and Part I. of this volume.



TABLE XXIII.—SHOWING THE AGES OF 756 MOTHERS OF TWINS.

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45	Total.
Collins . . . . .	3	53	76	71	28	8	1	240
M'Clintock . . . . .	2	23	45	41	17	1	...	129
M'Clintock and Hardy . . . . .	1	20	34	26	12	2	...	95
Chiari Braun and Spæth . . . . .	1	25	36	26	4	2	...	94
Statistics of 1855 . . . . .	3	28	46	58	52	11	...	198
Total . . . . .	10	149	237	222	113	24	1	756



But while there is this general accordance of results, an examination of the table at once reveals to the observer great differences between the general fertility of women and their fertility in twins. These differences will be the subject of further description ; in the meantime, I shall only adduce sufficient evidence to show that there is great difference, so much indeed as at once to demonstrate that the production of or fertility in twins is in woman not subjected to the same laws as fertility generally.

The mean age of 16,385 parturient women included in Dr. Collins' tables is 27 years. The mean age of 240 women in the same lists bearing twins is 29. The twin-bearer is older than the general run of bearers.

The number of twins born by women under and above thirty years in Dr. Collins' lists is 153 and 87, showing a majority of 66 on the side of the younger women, and thus a smaller proportional number of twins born of the young women than of all children born of the same.

But the data of Dr. Collins are not the best I can adduce to elucidate this point. His are derived from a class of cases submitted to selection, the conditions being all those connected with admission to the Lying-in Institution of Dublin. I bring forward data derived from an analysis of the whole legitimate births in Edinburgh and Glasgow in 1855. These show still more markedly and satisfactorily than the data of Collins, that a far larger proportion of twins than of children generally is born of elderly women. It is



easy to imagine reasons for the greater though similar difference shown by my statistics than by those of Collins; and there can scarcely be a doubt that mine are, in regard to this point, far more reliable than his. The mean age of 16,301 mothers of legitimate children in Edinburgh and Glasgow, in 1855, was above twenty-nine years. The mean age of 198 wives bearing twins was thirty-one years. The twin-bearer is here again older than the general run of bearers. In the same population the number of twins born by women under thirty years of age was 86; the number born by women above thirty years was 112,—showing a majority of 26 on the side of the elder women. Or, while of all births among these 16,301 wives three-fifths occurred among women under thirty years of age, there occurred only two-fifths of the twins among these younger women.\*

\* It is necessary to remark that the statements here given are not absolute or exact. For while the Dublin statistics include all the births at or near the full time, the statistics of Edinburgh and Glasgow include the same, with the exception of those born dead. But it is evident that were the figures to be exactly true, not only should the dead-born be included, but also all born in miscarriages or abortions. Until such comparative statistics are procured as include all births, mature and premature, living and dead, no statement, even of the comparative frequency of twin births, can be absolutely relied on; for fewer plural pregnancies come to maturity than pregnancies with single children. Chiari Braun and Spæth have shown that abortions are comparatively more frequent in plural pregnancies than in ordinary pregnancies.



## CHAPTER II.

THE INFLUENCE OF AGE ON WOMAN'S FERTILITY  
IN TWINS.

I HERE first produce a table formed by adding the data of Dr. Collins to my own, and showing the very remarkable result that, speaking generally, the older a mother is the more likely is she to have twins.

While, of mothers from fifteen to nineteen years of age, only every 189th had twins at a birth, mothers rising in age were more prolific in twins, till at the age of from thirty-five to thirty-nine years the climax of fertility in twins was reached, every forty-fifth woman producing two at a birth, or fully four times as many as the women under twenty. This statement of gradually-increasing fertility in twins does not, so far as the table shows, appear to hold good after the age of forty is reached. And I shall have presently to point out an analogous divergence from the same statement, but in an opposite direction, when I treat of the influence of primiparity in increasing twin-



TABLE XXIV.—SHOWING THE ACTUAL FERTILITY IN TWINS OF MOTHERS OF VARIOUS AGES.\*

Ages . . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54	55-59	Total.
Mothers . . . . .	1138	8550	10346	7667	3617	1237	118	12	1	32686
Mothers of Twins . . . . .	6	81	122	129	80	19	1	...	...	438
Proportion of latter to former is 1 in	189·6	105·5	84·8	59·4	45·2	65·1	...	...	...	74·6

\* The numbers in this table are got by adding together the numbers given by Collins and those extracted by myself from the registers of Edinburgh and Glasgow from 1855.

The following table shows the like facts ; only it is confined to the legitimate viable births in Edinburgh and Glasgow in 1855. The results also are similar, with this difference, that twin births are more frequent in the first than in the second quinquennial period :—

TABLE XXV.—SHOWING THE ACTUAL AND COMPARATIVE FERTILITY IN TWINS OF WIVES-MOTHERS OF VARIOUS AGES.

Ages . . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
Wives-Mothers . . . . .	376	3688	5037	3850	2407	840	103	16,301
Wives-Mothers of Twins . . . . .	3	28	46	58	52	11	...	198
Proportion of latter to former is 1 in	125	132	109	66	46	76	...	82



births.\* I venture, therefore, founding on the above observations, to state the law, that from the earliest child-bearing period, till the age of forty is reached,—that is, till a period when fecundity has become extraordinarily diminished—the fertility of mothers in twins gradually increases.

This twenty-fourth table, showing a fertility in twins gradually increasing with age, is almost exactly opposed to what our knowledge of the fecundity of women generally would lead us to expect. The general productiveness of a mass of wives is greatest at the commencement of the child-bearing period of life, and after that epoch gradually diminishes.

In the twenty-fourth and twenty-fifth tables twin-bearing mothers are compared with the whole mothers. I here produce a table of twin-bearing mothers compared with married women of corresponding ages. This twenty-sixth table is, in more respects than one, not very satisfactory. It cannot, I think, be expected to yield much. Yet its evidence is to the effect that when woman generally is most fecund she is least likely to produce twins. Between the ages of twenty and thirty years, fewest wives have twins. Before and after

\* Regarding twins as an abnormal birth, and the children as comparatively feebly organised or imperfectly developed, it may be found that their frequency in first births confirms an opinion, held not without reason, that a first-born child (not in a twin-birth) is, in general, more feeble than its followers. A first pregnancy is short. A first child is of comparatively light weight. A first egg of a fowl is smaller than those which follow.



that period of high general fecundity the special productiveness in twins increases. And this result is in general accordance with what we have already shown regarding productiveness of twins—that is, it is the opposite of what we know of general fecundity; moreover, it may find some special support from the evidence of the twenty-fifth table, and of the twenty-ninth.



TABLE XXVI.—SHOWING THE FERTILITY IN TWINS OF WIVES OF VARIOUS AGES.\*

Ages . . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Wives . . . . .	756	8874	14,622	14,579	11,871	10,506	7537
Wives-Mothers of Twins . . . . .	3	28	46	58	52	11	
Proportion of latter to former is 1 in	252·0	316·9	317·8	251·3	228·3	955·1	

\* The wives are estimated for Edinburgh and Glasgow for 1855. The mothers of twins are those of Edinburgh and Glasgow in 1855.



## CHAPTER III.

## INITIAL FERTILITY IN TWINS AT DIFFERENT AGES.

THE results arrived at in the former chapter are confirmed by a comparison of the initial fecundity of wives generally with the special initial fecundity in twins of the same women. The twenty-seventh table is unfortunately not large enough in numbers to afford results of a high degree of reliability. I believe that a more extensive collection will probably show a regular increase of initial productiveness of twins with increasing age. I leave it as it stands, showing that the wives married youngest have the fewest twins, and that there is an increase as age advances.



TABLE XXVII.—SHOWING COMPARISON BETWEEN WIVES NEWLY MARRIED, AND MOTHERS OF TWINS  
WITHIN TWO YEARS OF MARRIAGE.\*

Ages . . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
No. of Wives newly married . . . . .	700	1835	1120	402	205	110	75	4447
No. of Mothers of Twins within two years of marriage . . . . .	2	15	16	4	32	...	...	39
Proportion of latter to former is 1 in . . . . .	350	122	70	100	102	...	...	114

\* Here not only are the twins comparatively few, but also the numbers of wives are not real or actual, but estimated for Edinburgh and Glasgow in 1855. See page 26. The mothers of twins within two years of marriage are extracted from the registers of Edinburgh and Glasgow for 1855.



Fertility in twins is better or more justly contrasted with the fertility of fertile women than with the fecundity of a mass of wives both sterile and fertile. In the former case all the women brought into comparison bear children, and thus show their fecundity and fertility, and their adaptation for comparison, while in the latter case women fertile in twins are (as in Table XXVII.) compared with both women who are fertile and with those who are sterile. In the twenty-eighth table I establish a comparison between two sets of fertile women, the one bearing single children, the other bearing twins. What does this table show? It remarkably confirms the law already stated as to the increase of twins as fertile women grow older. And there is here seen a regular increase up till the age of forty is reached. Every 153d woman among the youngest fertile women bears twins (within two years after marriage); among the older women, from thirty-five to forty years of age, every forty-second woman bears twins within two years after marriage, or nearly four times as many.



TABLE XXVIII.—SHOWING COMPARISON BETWEEN MOTHERS WITHIN TWO YEARS OF MARRIAGE, AND MOTHERS OF TWINS WITHIN TWO YEARS OF MARRIAGE.\*

Ages . . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
No. of Mothers within two years of marriage . . . . .	306	1661	849	253	84	17	2	3172
No. of Mothers of Twins within two years of marriage . . . . .	2	15	16	4	2	...	...	39
Propn. of latter to former is 1 in	153	111	53	63	42	...	...	81

\* In this table the figures may with justice be regarded as the actual numbers (not estimated) in Edinburgh and Glasgow in 1855.



TABLE XXIX.—SHOWING COMPARISON BETWEEN MOTHERS TWO YEARS MARRIED AND UPWARDS, AND  
MOTHERS OF TWINS TWO YEARS MARRIED AND UPWARDS.

Ages . . . . .		15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
No. of Mothers two years married and upwards . . . . .		70	2027	4188	3597	2323	823	101	13,129
No. of Mothers of Twins two years married and upwards . . . . .		1	13	30	54	50	11	...	159
Propn. of latter to former is 1 in		70	156	139	66	46	75	...	82



I here interpolate another (twenty-ninth) table, which is supplementary to the twenty-eighth. It is composed of all mothers except those who have borne children within the first two years of marriage. Its evidence is to the same general effect as that of the preceding, only it seems to show the increased fertility in twins of the youngest women—a circumstance to which I have already called attention.

The especial value of these tables of initial fertility, and of subsequent fertility at various ages, lies in their eliminating entirely, or almost entirely, the disturbing element of the number of the pregnancy, and leaving evidence of the effects of age simply. In short, they contribute to the demonstration of the law of the influence of age stated in the second chapter of this part.



## CHAPTER IV.

THE RELATION OF THE FREQUENCY OF TWINS TO THE  
NUMBER OF THE MOTHER'S PREGNANCY.

IF it be true that the older a woman is who still retains a degree of fecundity, the more likely is she to bear twins, then we should, without further data, guess that twins were comparatively more frequent in late than in early pregnancies. And this is confirmed by an investigation of the subject. In the following table are given the number of children born in Edinburgh and Glasgow in 1855 in first and subsequent pregnancies, and beside them are placed for comparison the number of twins born in the same. A glance at the table shows that up to the ninth pregnancy, far beyond the average number of pregnancies, and as far as we have considerable numbers to guide us, the proportional frequency of twins increases with the number of the pregnancy. To this general statement there is an exception in the case of first pregnancies. Woman has apparently an increased chance of bearing twins in her first pregnancy, which leads to a disorder of the general rule above stated. With this notable exception, the rule holds manifestly good, at least till the ninth pregnancy is passed. After the ninth pregnancy, the table, from the smallness of the numbers and the irregularity



of the results, cannot be much relied on. Yet it shows, on an average, a still increasing fertility in twins as the number of the pregnancy advances. According to Table XXX., among a thousand child-bearing women pregnant for a first, second, or third time, as these pregnancies happen to occur in our population, nine only

TABLE XXX.—SHOWING THE ACTUAL AND COMPARATIVE NUMBER OF TWINS BORN IN FIRST AND SUBSEQUENT PREGNANCIES.\*

No. of Pregnancy.	No. of Children.	No. of Twins.	Percentage of latter to former.
1st	3722	45	1·21
2d	2893	19	·66
3d	2534	24	·94
4th	1982	19	·96
5th	1543	19	1·23
6th	1221	18	1·47
7th	848	17	2·00
8th	641	14	2·18
9th	425	14	3·29
10th	222	3	1·35
11th	152	2	1·31
12th	61	3	4·92
13th	34		
14th	11		
15th	6		
16th	2	1	50·00

\* It is necessary to note a source of error that exists in this table. In the third column are given the twins registered in 1855, and no other twins are supposed to have been in the family previously. The registers only give the number of previous viable children, whether twins or not; they do not give the number of pregnancies. These are taken directly from the number of viable children.



will produce twins ; among a thousand similar women in fourth, fifth, and sixth pregnancies, there will be twelve twin births ; among a thousand similar women in seventh, eighth, and ninth pregnancies, there will be twenty-four twin births ; and among a thousand similar women in tenth, eleventh, and twelfth pregnancies, there will be twenty-five double births.

It may then be stated that, after passing the first pregnancy, a woman's chance of bearing twins increases with each subsequent pregnancy.

The broad statement, that multiparous women are more likely to have twins than primiparous, has been made by Chiari Braun and Spæth, and by Hugenberg. But this is only an imperfect development of the general statement just enunciated.

I have hitherto carefully abstained from giving this general statement the dignity of a law ; for it may be only a coincidence resulting from the circumstance that age of mothers increases as the number of the pregnancy increases. The law of increased frequency of twins with advancing age may afford the explanation of the increased frequency as the number of the pregnancy advances. It remains to be determined, then, whether this general statement be a law or only a corollary to the law of age.

I now present a table (XXXI.) which is so constructed from the data at my command as to avoid error from the influence of age, women of the same age, but of different pregnancies, being compared. In it various adjacent pregnancies and ages are thrown



TABLE XXXI.—SHOWING THE COMPARATIVE FREQUENCY OF TWINS IN DIFFERENT SETS OF PREGNANCIES OF WIVES OF THE SAME AGE.

Mother's Age . . . . .	25 to 29.			30 to 34.			35 to 39.		
	No. of Children.	No. of Twins.	One in	No. of Children.	No. of Twins.	One in	No. of Children.	No. of Twins.	One in
Pregnancies 2d, 3d, & 4th .	3235	20	162	1628	19	86	568	9	63
Pregnancies 5th, 6th, & 7th	766	6	128	1568	27	58	993	17	58
Pregnancies 8th, 9th, & 10th	28	1	28	283	7	40	616	19	32



together in order to attain considerable numbers, with a view to reaching a trustworthy conclusion, and these collocations diminish the value of the table.

Yet an inspection of it will, I believe, convince the observer of the influence of the number of the pregnancy. And a comparison of this table with those previously given demonstrating the influence of age, leaves no doubt that the increase of twins with the number of the pregnancy is greater than could be accounted for by the error introduced by using quinquennial periods—that is, by the possible accumulation of the more advanced pregnancies in the last years of the quinquennial periods contrasted.

The increased frequency of twin-bearing as the number of the pregnancy increases may therefore be now regarded as a law of the production of twins.

In order to the more complete discussion of the influence of the number of the pregnancy on the frequency of twin births, I produce another table (XXXII.) whose interesting results throw light on the subject.

The third column of this table shows how a hundred twin births are distributed according to the number of the pregnancy. It is evident that, speaking generally, twinning becomes rarer as the number of the pregnancy increases ; and at the top of the list, far surpassing all the rest, is the first pregnancy with the large number of nearly 23 per cent of all twin births. Actually, then, twinning diminishes as the number of the pregnancy increases.



TABLE XXXII.—SHOWING THE FREQUENCY OF TWIN BIRTHS AND OF ALL BIRTHS IN FIRST AND SUBSEQUENT PREGNANCIES.\*

Number of Birth in Family.	Number of Twin-Bearing Mothers.	Percentage of Twin-Bearing Mothers.	Percentage of Mothers.
1st	45	22·7	22·8
2d	19	9·6	17·7
3d	24	12·1	15·5
4th	19	9·6	12·1
5th	19	9·6	9·4
6th	18	9·1	7·4
7th	17	8·6	5·2
8th	14	7·1	3·9
9th	14	7·1	2·6
10th	3	1·5	1·3
11th	2	1·0	·9
12th	3	1·5	·3
13th			
14th			
15th			
16th	1	0·5	·01

But we must go further into the matter, and we find that, as the number of the pregnancy increases, so the number of all births, single and plural, diminishes. The fourth column in Table XXXII. shows how a hundred births are distributed among the pregnancies in some of which twins occurred. There is a regular diminution as the number of the pregnancy increases ; and, in order to comprehend the relation of twin-bearing to child-bearing generally, this column must

\* This table reads as follows :—22·7 per cent of twin-bearing mothers are bearing their first viable children ; 22·8 per cent of all mothers are bearing their first viable children ; and so on.



be contrasted with the preceding. Doing so, we find the first four pregnancies forming a contrast to the subsequent pregnancies. In the former, or first four pregnancies, the proportion of twinning in a hundred twinning is smaller than the proportion of child-bearers in a hundred: in the latter, or the fifth and subsequent pregnancies, the proportion of twinning in a hundred twinning surpasses the proportion of child-bearers in a hundred, and the preponderance goes on increasing from the fifth at least as far as the ninth pregnancy.

In this chapter I have shown that—

1. The actual number of twins born in different pregnancies decreases as the number of the pregnancy increases.

2. The comparative number of twins born in different pregnancies increases as the number of the pregnancy increases.

3. The increase of the comparative number of twins with the number of the pregnancy does not appear to hold good with the first pregnancy as compared with the three immediately subsequent pregnancies; women in their first pregnancies being more likely to bear twins than in those immediately subsequent.



## CHAPTER V.

## THE SIZE OF FAMILIES IN WHICH TWINS OCCUR.

It is very desirable to know what influence the having of twins has upon women's fertility. Do women having twins bear larger families than those never getting but one at a birth? To this question one naturally gives an affirmative answer.\* Of course, if women's subsequent fertility be not affected by twin-bearing, there will be just an excess above ordinary families of a unit for every pair born in a family, and I daresay I am right in saying this is the popular belief. It receives some sort of support from the circumstance that twins are relatively more frequently additions to an already existing considerable family than they are either the first of a family or additional to an already existing small family. Moreover, twins occur with greater proportional frequency in elderly

\* In this chapter no consideration is taken of the survival of the children forming the family. No doubt, twins in a family will diminish the chances of survival. Although it is probable that races may differ as to amount of twinning without difference in general fertility, it appears to me that twinning may come to be a good test of excessive fertility or of the imperfection of the children.



than in younger women, and are therefore certainly less likely, and perhaps less liable, to interfere with further or subsequent productiveness than if they occurred chiefly among the younger women.

To contribute to the solution of this interesting question, I have framed the following table (XXXIII.) It also appears to support the affirmative response to the question just given. It shows that the average size of families of women married the same number of years is greater in the twin-bearing than in ordinary families, counting down to the birth of the twins. This is all that I can say in favour of the view or supposition that twin-bearing women have larger families than their neighbours. But the view is very far from being so demonstrated true.

TABLE XXXIII.—SHOWING THE AVERAGE SIZE OF FAMILIES AFTER DIFFERENT DURATIONS OF MARRIAGE IN MOTHERS GENERALLY, AND IN MOTHERS BEARING TWINS.\*

No. of Years Married.	Under 5.	5-9.	10-14.	15-19.	20-24.	25-29.
Average size of Families .....	1·699	3·940	6·063	7·967	9·868	13·075
Average size of Twin Families	2·523	4·936	7·397	9·793	9·533	

The thirty-third table seems to me certainly to show that twinning has retrospectively no connection

\* The mothers in this table are those only who continue fertile up till the different durations of marriage.



with a diminished degree of fertility of woman ; that a woman destined to have twins at some future time is in the meantime as fertile as any other ; that twinning does not occur to supplement pre-existing deficiency. So far from this being the case, the table shows that twin-bearing women have on the whole been already more fertile than their neighbours ; that the twins come as additions to families already numbering above the average. For, keeping in mind that twins are comparatively most frequent in the latter pregnancies of women, it is easily seen that while, in the first column, that is among women under five years married, or in the earlier pregnancies of women, twinning does not add so much as a unit to the average size of the family in which the twins occur ; in the next column, or among women from five to ten years married, twinning does add a unit ; in the third and fourth columns, containing the greater proportional number of twin-bearers, the twinning adds more than a unit ; or, in other words, the women, even if they did not have twins, have borne larger families than their neighbours. The table shows then that the great majority of women having twins are already more prolific than usual. The last column of women is evidently exceptional ; it relates to women from twenty to twenty-four years married, and who have passed (as shown in other ways) their ninth pregnancy ; and, as already shown, it is founded on a very narrow foundation of figures.

But all this is only nearly a complete demonstration



of the affirmative, that twin-bearers are more fertile than others. No doubt a twinner is in that birth more fertile than a uniparous woman. No doubt, as just shown, a twin-bearer is, counting up till the time of twinning, more fertile than a woman bearing one at a time. But the real question is not answered. Does a woman who has finished bearing a family, and has in that family had twins, produce more or fewer than a woman always uniparous? In other words, do twin-bearing women, who have shown all the fertility of their lives, produce larger or smaller families than women uniformly uniparous? This is the real question. To it the above-mentioned arguments do not supply a conclusive answer; and I regret to say I must leave it unanswered. Only I admit that the affirmative is probably the true answer. To procure a reliable solution, mothers must be compared who have borne their last children. I have no such data. Table XXXIII. is not a table of women who have borne their last children. It carries the women of a population down only to their children born and registered in 1855.

In bringing this part to a conclusion, I may remark that the chief results of it appear to me to be well established by the evidence. Yet I cannot but feel that a larger accumulation of data would have added to their security and firmness.

It is interesting, first of all, to note that twin-



bearing is not an accident, that it is subjected to laws of which we now have a glimpse. A philosopher might have fancied twinning to be the result of the act of conception taking place on the rupture of a Graafian vesicle fortuitously containing a double ovum or two ova, or on the rupture of two Graafian vesicles accidentally matured simultaneously ; in other words, that twinning was the result of some transcendental primordial energies in the ovary, whose products might be attributed to chance, because their origin was not only unknown but inscrutable by any known means of investigation. The data, as here arranged, compared, and reasoned upon, seem already to carry us so far as to remove twin-bearing from the category of the accidental, and to indicate to us laws of their occurrence which may be steps in the ladder of ascent to higher knowledge and wonderful discovery.

In the course of this part I have attempted to demonstrate the following conclusions :—

1. The largest number of twins is produced by women of from twenty-five to twenty-nine years of age ; and on each side of this climax of fertility in twins there is a gradually increasing falling off in their number as age diminishes on the one side and increases on the other. (Table XXIII.)

2. Twins are not regularly distributed among births generally ; their production, therefore, is not



subjected to the same laws as govern ordinary fertility.

3. The mean age of twin-bearing mothers is greater than that of mothers generally.

4. Twins increase in frequency as mothers become older (Table XXIV.) This forms a striking contrast to the fecundity of a mass of wives (not mothers) which diminishes as their age increases. It accords, however, with the law of intensity of fertility of fertile women.

5. Newly-married women are more likely to have twins the older they are. (Tables XXVII. and XXVIII.)

6. While the fecundity of the average individual increases with age till twenty-five is reached, and then gradually diminishes, there is some probability that the opposite is true, so far as regards twins alone, fertility in twins being greatest when fecundity is least, and *vice versa*. (Tables XXV. XXVI. XXIX.)

7. The actual number of twins born of a mass of women in different pregnancies decreases as the number of the pregnancy increases. (Table XXX.)

8. The number of twins, relatively to the number of children born in different pregnancies, increases with the number of the pregnancy. In other words, a woman is more likely to have twins in each succeeding pregnancy than in the former pregnancy. The first pregnancy forms an exception to this rule. (Tables XXX., XXXI., and XXXII.)



9. In an individual, twin-bearing is of course a sign of high fertility at the time. It also, in a mass of women, shows a high amount of fertility, at least till the time of the birth of the twins. (Table XXXIII.)

10. It is probable, though not proved, that twin-bearing women have larger families than women uniformly uniparous.



## PART IV.

### ON THE LAWS OF THE FERTILITY OF WOMEN.

WHEN concluding my account of fecundity, including the question of the age at which women are most likely to have children after marriage, I said that I could not advance further without encroaching on another topic—viz. the fertility of marriage; or, as marriage is scarcely admissible as a term in physiology, the subject may be designated “sustained fecundity,” or the laws of the fertility of women cohabiting with men during the child-bearing period of life. It is this subject which I propose now to enter upon. So far as I know, very little is ascertained in this department of physiology. The writings upon it are for the most part to be found in the works of political economists, and are chiefly confined to the single question of the rate of increase of a population under varying circumstances. To illustrate this topic, which is one of little interest to the physiologist, data are numerous and abundant. But when the writers referred to attempt to go deeper into the fundamental laws of the fertility of women, having very scanty materials and using them without care, they arrive at scanty results, which are either positively erroneous or of little value.

“The statistics,” says Major Graham, registrar-



general for England, “ of a country in which the age of a mother at marriage, and at the birth of her children, is not recorded, must always remain imperfect, and leave us without the means of solving some of the most important social questions.”\* These data were secured for the first year of the registrations in Scotland. The results to be now described are derived from a study of a part of these registers—namely, those of Edinburgh and Glasgow for 1855, and are founded on an analysis of 16,301 families of wives.

\* *Registrar-General's Report for 1845*, p. xiv. (England.)



## CHAPTER I.

THE FERTILITY OF THE WHOLE MARRIAGES IN A  
POPULATION.

ON this subject much has been written, in latter times chiefly by Malthusians and anti-Malthusians, to whose works I refer generally. Elaborate comparisons are made between the fertilities of marriage in different countries; and there are exhibited variations to so great an extent, that they appear themselves to show the worthlessness of the data and of the comparisons instituted, at least in a physiological point of view. In illustration, I may refer to the variations described by M. Benoiston de Chateauneuf,\* in a paper on the intensity of fecundity in Europe at the commencement of the nineteenth century. The highest figure is derived from some villages in Scotland, where there are asserted to be six or seven children to a marriage, while his lowest figure is 2·44, the alleged productiveness of some marriages in Paris.

We shall restrict our view to Great Britain; and we find the method, generally followed, of estimating the fertility of marriage, to be the very old and simple one of dividing the number of legitimate births in any year by the number of marriages. “In 1861,” says

\* *Annales des Sciences Naturelles*, tome ix. 1826.



Dr. Stark,\* “for every marriage which occurred in Scotland there were born 4·64 legitimate children; that is to say, 464 legitimate children were born to every 100 marriages. During the same year, in England, only 3·89 legitimate children were born to every marriage, or 389 legitimate children to every 100 marriages.” This is an exemplification of the ordinary method of calculating; and it is evident that the result derived is of not the slightest value as a contribution to the science of fertility. For, besides including marriages of all durations and at every fecund age, also second and third marriages, it includes many marriages at ages when fecundity has entirely disappeared. It is impossible, indeed, to state what is the exact relation between the number of marriages in a population in any year and the number of legitimate children born in the same year, with a view to any physiological result. This aspect of the statement is, however, well worthy of being pointed out, because authors of respectability, whom it is needless to name, refer to and use these figures as exhibiting the fertility of continued married life in England and Scotland. Malthus was well aware of the real meaning of these figures—of the fact that they merely show the relative frequency of marriage-ceremonies and births in a population. “The rule,” he says,† “which has been here laid down, attempts to estimate the prolificness of

\* *Seventh Detailed Annual Report for 1861*, published in 1865, p. xviii. (Scotland.)

† *Essay on the Principle of Population*, vol. ii. p. 6.



marriages, taken as they occur; but this prolificness should be carefully distinguished from the prolificness of first marriages and of married women, and still more from the natural prolificness of women in general, taken at the most favourable age. It is probable," he adds, "that the natural prolificness of women is nearly the same in most parts of the world; but the prolificness of marriages is liable to be affected by a variety of circumstances peculiar to each country, and particularly by the number of late marriages."

As a corollary from the preceding data, of value only in proportion to their value, it may be stated that the average duration of fertility in married women (including those who do not bear children) is about  $7\frac{1}{2}$  years. For, as the intervals between marriage and the birth of a child, and between the births of successive children, is, on an average, 20 months, and as there are about  $4\frac{1}{2}$  children to each marriage, we have about  $7\frac{1}{2}$  years, counting from marriage, spent in producing that number.

British authors, as Graunt, Short, Malthus, Sadler, Senior, and those of later date, name 4,  $4\frac{1}{2}$ , or 5, as the fertility of marriage. Malthus, founding on such data, gives a wife four children produced within eight years, a statement which cannot be passed over without the obvious remark that Malthus, so calculating, utterly neglects the force of the wise words which we have just quoted from his work.

Making use of the Swedish returns, Major Graham



has, in his last annual report, published results obtained in a novel manner. I quote his words : \*—

“ The marriages in a calendar year give rise to births, which are registered year after year for 20 years. The births to the 167,723 marriages in the year 1859 could only be determined by following the families and counting all the children unto the end. The division of the sum of the children by the marriages would accurately express the fecundity, as it has been called, of marriages. If the annual marriages do not increase or decrease in number through a series of years, the division of the annual births by the annual marriages of the same years expresses the fecundity pretty accurately ; but the marriages in England are increasing rapidly ; consequently the 740,275 births registered in the year 1864 must be divided by the marriages of some earlier year to get an approximation to the fecundity. As the age of the mothers is unfortunately not recorded, the interval in England is unknown which intervenes between the mean age of marriage and the mean age of the mothers when their children are born ; otherwise that interval would indicate the calendar years with which the births of the year 1864 should be compared.

“ But the interval in Sweden between the mean ages of mothers at marriage (25·8 years) and their mean age at the births of their children (31·7) is six years ; and the interval in England cannot differ much

\* *Twenty-seventh Annual Report of the Registrar-General* (England), p. xix.



from six years.\* Hence, if the legitimate births of given years are divided by the marriages of six years' earlier date, the quotient will be the proportion of children to a marriage within close limits. In England the births thus determined to a marriage were 4·255, 4·301, 4·304, in the years 1862, 1863, and 1864. In Scotland the births in 1862 to the average marriages of six years' earlier date (1855, 1856, and 1857) were 4·694."

\* The table published by Dr. Stark, in the *Eighth Detailed Annual Report* (Scotland), with a view to shew the influence of cold on fecundity, seems to me to show, that Major Graham's argument from Sweden to England is not well founded. The differences there shown to exist between the two countries are probably greater than Major Graham knew when he wrote the passage quoted above. The mean age of first marriages in England is 24·6 years for females (*Census of Great Britain for 1851*, vol. i. p. xxxi.). The mean age at the birth of children is, in Collins' collection, 27 years; in Edinburgh and Glasgow in 1855, a little above 29 (see pages 6 and 10).



## CHAPTER II.

FERTILITY OF THE WHOLE FERTILE MARRIAGES IN A  
POPULATION, AT A GIVEN TIME.

I HAVE nothing satisfactory to offer as to prolific marriages to contrast with the statements given concerning all marriages. Dr. Lever\* says, that “the average number of children consequent upon a prolific (not every) marriage is shown to be rather more than  $5\frac{3}{4}$ , but not amounting to 6.” This is given without any authority stated or evidence detailed, and I know not what value to ascribe to it. In a physiological point of view, its value must be scarcely appreciable; for no allowance is made for the duration of the marriage, nor for the age of the woman at the time of the ceremony.

In St. George’s-in-the-East, London, the average number of children consequent on the prolific marriages was 5·33 to each marriage.† That is, 5·33 is the average number of children that has been born in all the families in a place at a given time. It tells nothing con-

\* *On Organic Diseases of the Uterus*, p. 5.

† *Quarterly Journal of the Statistical Society of London*, vol. xi. 1848, p. 235.



cerning the average number in completed families, or in still-growing families,\* or in existing or still undispersed families.

Franklin says that the females in America have “one with another, eight children to a marriage;”† almost certainly a great exaggeration, especially as he does not even state, as a condition, that the marriages were prolific.

\* Some interesting facts regarding the fertility of Esquimaux women are to be found in Robertson’s *Essays and Notes on Physiology and Diseases of Women*, p. 53.

† Sadler. *Law of Population*, vol. ii. 495.



## CHAPTER III.

ANNUAL FERTILITY OF THE MARRIED WOMEN OF CHILD-  
BEARING AGE IN A POPULATION.

SEEING the inexactness of the statements of which those just given are an example, Dr. Stark has adopted another method of arriving at the comparative prolificness of marriages in England and Scotland. “In 1861,” says he, “when the census was taken in England, the number of wives at the child-bearing ages—viz. 15 to 45—was 2,319,649; and as the number of legitimate children born during the year amounted to 652,249, this gives the proportion of one legitimate child for every 3·55 wives at the ages of 15 to 45 in the population; or, in other words, every 355 wives in England, at these ages, gave birth to 100 children during the year. In Scotland, during the same year, there were 305,524 wives between the ages of 15 and 45 years; and as 97,080 legitimate children were born during the year, this gives the proportion of one legitimate child for every 3·14 wives at these ages in the population; or, in other words, every 314 wives in the population of Scotland, at these ages, gave birth to 100 legitimate children during the year.”\*

\* *Seventh Detailed Annual Report* (Scotland), p. xix.



While for every marriage in 1861 there were born in the same year in Scotland 4·64 legitimate children ; every 3·15 wives between 15 and 45 in Scotland in the same year produced one legitimate child. Of 54,408 wives in Edinburgh and Glasgow in 1855 between 15 and 44 years of age inclusive, 16,290 bore children fit for registering ; or, one child was born to every 3·3 wives aged from 15 to 44.

If we adopt these latter statements, we must take care to note that they do not give the fertility of the whole marriages in a population, as the older and former statements in Chapter I. do. These latter give the annual productiveness of a mass of married women in our populations. The results of the two methods of computing the fertility of marriage cannot be contrasted, for each is concerned with an entirely different topic from the other.



## CHAPTER IV.

THE SIZE OF THE FAMILIES IN A POPULATION AT  
A GIVEN TIME.

IN order to prevent confusion with calculations which are found in writings on population, I here insert an extract from Major Graham's writings on the subject of this chapter. Analysing a part of the English returns he comes to the conclusion that the average number of children to an existing family is 2·26.\*

“The number (says he) of children resident with their parents was 93,788 ; and there were 2·26 children on an average to each family, or 4·26 children and parents, including the father and mother, to each family of this class. Striking off the families consisting of husband and wife, sole, there remain 31,896 pairs having with them at home 93,788 children ; that is 2·94 children to a family, or 4·94 children and parents to a family. A fourth part of the families had four children or more at home, and these families of parents and children consisted of seven persons on an average.”

\* *Census of England and Wales, 1861 — General Report*, vol. iii. p. xi.



## CHAPTER V.

FERTILITY OF THE WHOLE MARRIAGES IN A POPULATION  
THAT ARE FERTILE AT A GIVEN TIME.

IN Edinburgh and Glasgow in 1855 there were 16,393 wives who bore first or subsequent children. Of these the necessary data are given in 16,301 cases. These 16,301 mothers had produced 60,381 children; or 3·7 children constituted the average production of each mother. In other words, excluding the large class of wives sterile in 1855, we have 3·7 as the average number of children (surviving or not surviving) in each family that increased in 1855.

To compare with the above result, we may observe 16,414 women delivered in the Dublin Lying-in Hospital during Dr. Collins' mastership, who had borne 53,458 children, whose families, on an average, numbered 3·25; also 6634 women delivered in the same hospital during the period reported on by Drs. M'Clinck and Hardy, who had borne 20,680 children, whose families, on an average, numbered 3·12.

As there can be no doubt that these 16,301 families are a fair sample of all the growing families in Edinburgh and Glasgow, it appears that the average size of growing families existing at a particular time in our population is between 3 and 4; and if it be



true that, on an average, children are born with an interval not exceeding twenty months, then all mothers child-bearing at any particular time have been on an average less than seven years fertile. It is to be remarked that this statement concerns only the families of wives-mothers child-bearing at a particular time (*i.e.* in 1855), and is not to be compared with the corollary to Chapter I., which includes all families, and especially the mass of completed families.

The accompanying Table (XXXIV.) shows the data upon which these statements are founded. It, in addition, gives the percentage of children (surviving or not) in families of different numbers, that increased in 1855.



TABLE XXXIV.—SHOWING THE NUMBER AND PERCENTAGE OF MOTHERS BEARING RESPECTIVELY 1ST, 2D, AND 3D CHILDREN, AND SO ON ; ALSO PERCENTAGE OF CHILDREN IN STILL GROWING FAMILIES OF DIFFERENT NUMBERS.

Number of Child.	Number of Wives-Mothers.	Percentage of Wives-Mothers.	Percentage of Children.
1	3722	22·83	6·16
2	2893	17·74	9·58
3	2534	15·54	12·59
4	1982	12·16	13·13
5	1543	9·46	12·77
6	1221	7·49	12·13
7	848	5·20	9·83
8	641	3·93	8·49
9	425	2·60	6·33
10	222	1·36	3·67
11	152	·93	2·77
12	61	·37	1·21
13	34	·20	·732
14	11	·06	·255
15	6	·03	·149
16	2	·01	·053
17	2	·01	·056
18	1	·006	·029
19	1	·006	·031



## CHAPTER VI.

THE FERTILITY OF FERTILE MARRIAGES LASTING DURING  
THE WHOLE CHILD-BEARING PERIOD OF LIFE.

THIS subject may be stated in the form of a question: How many children does a fertile woman produce, living in wedlock from fifteen to forty-five years of age? The only collection of data known to me, which can throw light on this point, is that published in the "Report to the Council of the Statistical Society of London, from a Committee of its Fellows, appointed to make an investigation of the state of the poorer classes in St. George's-in-the-East."\* In that district there were found eighty mothers married at ages varying from fifteen to nineteen, and who had lived in wedlock at least thirty-one years. These fertile wives, having lived nearly all the child-bearing period of life in wedlock, had borne on an average 9·12 children.

There are evident sources of inexactness in the above very limited data which tend to diminish the average fertility; and it will be as near the truth to state ten as the average fertility of fertile marriages lasting during the whole child-bearing period of life.

\* *Quarterly Journal of the Statistical Society*, August 1848, vol. xi.



The conclusions given in further parts of this paper will show that the figure of ten children, for thirty years of child-bearing life, is not indicative of each mother having borne a child every third year. The fertility, while it lasts, will be shown to be much intenser than this. The average interval between births of successive children is hereafter shown to be twenty months, which gives about seventeen years as the average duration of fecundity in a fertile woman living in the married state all the child-bearing period of life.

In his work on *Abortion and Sterility* Dr. Whitehead gives no data which I can properly collate with those just given. After stating his belief that the actual duration of the child-bearing period in the female of this climate is about twenty years, he adds that a woman, under favourable circumstances, has in that period twelve children. But as this includes abortions and premature deliveries, which he estimates at  $1\frac{1}{2}$  for each individual, the figure 12 has to undergo that reduction for comparison with 10, and the approximation is very close.

Sadler states as a fact, "that marriages, on the average, are only fruitful for about a third part of the term of possible fecundity."\* But he nowhere, so far as I know, affords any evidence of this statement, and I therefore attach to it no importance.

\* *Law of Population*, vol. ii. p. 276.



## CHAPTER VII.

THE FERTILITY OF PERSISTENTLY FERTILE MARRIAGES  
LASTING DURING THE WHOLE CHILD-BEARING PERIOD  
OF LIFE.

THIS subject may also be conveniently stated in the form of a question : How many children does a fertile woman produce, living in wedlock from fifteen to forty-five years of age, and bearing children periodically up to the end of that time ?

To this question I cannot give at once an answer founded on sufficient data ; and I shall invert my usual mode of proceeding, stating the conclusion—namely, that fifteen at least is the average number of children borne by a persistently fertile female in thirty years, before giving the reasons for it. These are as follows :—A persistently fertile woman, at all ages, is found to have borne one child about every two years ; the average fertility of fifteen mothers who have had each twenty-six years of persistently fertile life is thirteen ; the thirty-seventh table, to be hereafter given, showing an excess of fertility on the part of those long persistently fertile, or bearing children in the year of counting, would give sixteen as the proportional fertility of thirty years of persistently fertile marriage, calculating from



the actual values given for the other results in the table. The deficiency of actual facts for settling this point is to be seen in the next Table (XXXV.), where the number of women bearing children when above twenty-six years married is only seven.

On this subject Allen Thomson makes the following statement, which is remarkably accurate, seeing that it is apparently not founded on any analysis of documents. "A healthy woman," says he,\* "bearing during the whole time, and with the common duration of interval, may have in all from twelve to sixteen children, but some have as many as eighteen or twenty."

\* *Todd's Cyclopædia of Anatomy and Physiology*, vol. ii. p. 478.



## CHAPTER VIII.

FERTILITY OF PERSISTENTLY FERTILE WIVES AT  
DIFFERENT YEARS OF MARRIED LIFE.

THE following Table (XXXV.), from the 1855 Edinburgh and Glasgow data, gives at a glance the rate of yearly-increasing production of wives-mothers who are still fertile—that is, who produced a living child in the year of our census or counting. It is framed by adding together the whole children born of mothers having different durations of marriage, and dividing the sum by the number of mothers corresponding to each duration of marriage. The results will be found, on the whole, to tally pretty closely with those given in Table XXXIX. It is easy to account for the differences between the two tables. In the latter table the wives arrived at different numbers of progeny are collated and compared, while in the former the wives arrived at different durations of marriage are collated and compared. The table requires no further explanation ; it is easily read.



TABLE XXXV.—SHOWING THE AVERAGE NUMBER OF CHILDREN THAT HAVE BEEN BORN AT THE COMPLETION OF EACH YEAR OF PERSISTENTLY FERTILE MARRIAGE.

Duration of Marriage.	Number of Wives- Mothers.	Number, of Children.	Average to each Mother.
	16,301	60,381	3·70
1 year married and under	3,172	3,336	1·06
2 years                    "	1,223	2,090	1·70
3       "                   "	1,540	3,195	2·07
4       "                   "	1,248	3,229	2·58
5       "                   "	1,193	3,645	3·05
6       "                   "	1,122	3,959	3·53
7       "                   "	870	3,414	3·92
8       "                   "	733	3,225	4·40
9       "                   "	719	3,447	4·79
10       "                   "	761	4,021	5·28
11       "                   "	624	3,502	5·61
12       "                   "	520	3,134	6·03
13       "                   "	441	2,878	6·53
14       "                   "	393	2,698	6·86
15       "                   "	372	2,659	7·15
16       "                   "	293	2,248	7·67
17       "                   "	240	1,918	7·99
18       "                   "	198	1,647	8·32
19       "                   "	177	1,541	8·71
20       "                   "	142	1,303	9·17
21       "                   "	115	1,116	9·70
22       "                   "	80	790	9·87
23       "                   "	56	557	9·95
24       "                   "	39	415	10·64
25       "                   "	8	95	11·87
26       "                   "	15	195	13·00
27       "                   "	2	25	12·50
28       "                   "	3	42	14·00
29       "                   "	1	14	14·00
30       "                   "	1	13	13·00



## CHAPTER IX.

FERTILITY OF FERTILE WIVES AT DIFFERENT PERIODS  
OF MARRIED LIFE.

WITH a view to comparison with the results given in Table XXXV., I have prepared the following Table (XXXVI.) from the data of St. George's-in-the-East, already referred to. The circumstances in which these data were collected, and their paucity, do not justify me in ascribing to them a value equal to those given in Table XXXV., nor do I think they are well adapted for the purpose of the comparison for which they are adduced. But I know no other to refer to.

As in the Report of the Committee of the Statistical Society, the periods are counted from the birth of the first child; I have added to them 17 months ( $1\frac{5}{12}$ ths year), the average interval between marriage and birth of a first child, with a view to make the table more easily contrasted with Table XXXV.

The direct results of this table are given in the figures, and require no statement. But comparing it with the preceding table, we observe that, as is easily understood, the differences between the fertile and the persistently fertile increase as the duration of marriage increases; and that, while the numbers of the children



of fertile women is about a third of the years of duration of marriage, the numbers of the children of persistently fertile women is about a half of the years of duration of marriage. In other words, if these tables are at all trustworthy, we may guess that the number (surviving or not) of a fertile married woman's family is about a third of the number of years since her marriage. But if, in addition to knowing that the married woman has a family, we know that she has just had an addition to her family, then we may guess that the number of her family is about a half of the number of years since her marriage.

TABLE XXXVI.—SHOWING, FROM THE DATA OF ST. GEORGE'S-IN-THE-EAST, THE FERTILITY OF FERTILE WIVES AGED FROM 15 TO 45 YEARS.

Years Married.	Mothers.	Children.	Average of each Mother.
$2\frac{5}{12}$	56	59	1.05
$3\frac{5}{12}$	60	88	1.46
$4\frac{5}{12}$	54	99	1.83
$5\frac{5}{12}$	66	184	2.79
$6\frac{5}{12}$	57	163	2.86
$7\frac{5}{12}$	60	196	3.26
$8\frac{5}{12}$	76	269	3.54
$11\frac{5}{12}$	254	1178	4.64
$16\frac{5}{12}$	215	1319	6.13
$21\frac{5}{12}$	148	1075	7.26
$26\frac{5}{12}$	44	353	8.02

From the same London data I have also framed the following table, without doing any apparent violence to them, and with a result which is extremely



interesting. The student will observe that beside the data from St. George's-in-the-East I have placed corresponding data extracted from the Edinburgh and Glasgow registers of 1855. The comparison of the fertility of a set of fertile wives—that is, all wives who have borne children some time during their still-continuing married lives—with that of a set of persistently fertile wives—that is, exclusively, of wives bearing at the ends of the periods under consideration (that is, in this table, the end of their child-bearing lives)—is, as already said, marred, and loses value on account of the two sets being of very different numbers, different localities, and different populations. Taking it as it stands, we find that fertile women generally, living with husbands for sixteen years before the conclusion of child-bearing life, have an average family of about  $4\frac{1}{2}$ ; while persistently fertile wives—that is, wives bearing children at the end of their child-bearing lives—have an average family of  $11\frac{1}{2}$ . While fertile wives, married twenty-one years, before and up to the age of forty-five, have an average family of about 6; persistently fertile wives have an average family of  $10\frac{1}{2}$ . While fertile wives married for twenty-six years, before and up to the age of forty-five, have an average family of 8; persistently fertile wives, in the same circumstances, have an average family of about 14. While fertile wives, married for thirty-one years, before and up to the age of forty-five years, have an average family of 9; persistently fertile wives, in the same circumstances, have an average family which may be estimated at 16.



TABLE XXXVII.—SHOWING A COMPARISON OF THE FERTILITY OF MOTHERS AND OF  
PERSISTENTLY FERTILE MOTHERS.

		ST. GEORGE'S-IN-THE-EAST. —— Wives-Mothers.			EDINBURGH AND GLASGOW IN 1855. —— Wives-Mothers bearing Children at the end of Child-bearing Life		
Age at Marriage.	Duration of Marriage.	Number of Mothers.	Number of Children.	Average Fertility of each Mother.	Number of Mothers.	Number of Children.	Average Fertility of each Mother.
15-19	At least 31 years	80	730	9·12	...	...	16
20-24	At least 26 years	179	1418	7·92	6	83	13·83
25-29	At least 21 years	100	630	6·30	7	74	10·57
30-34	At least 16 years	25	115	4·60	4	46	11·50



In this Table (XXXVII.) it will be observed that the differences between the fertile and the persistently fertile are much greater than in the former (XXXV. and XXXVI.), a circumstance which is easily explained. For, in the latter, all the women have been long married, and the persistently fertile have had time to far outrun the average fertility of all the fertile. It must also be noted that all the women in the table are fertile at or near the end of the child-bearing period, a time at which, it will be hereafter shown, the intensity of fertility is greater than at any other.



## CHAPTER X.

DEGREES OF FERTILITY OF WIVES-MOTHERS OF  
FAMILIES OF DIFFERENT NUMBERS.

UNDER this head the first question that raises itself relates to the interval between marriage and the birth of the first child. In Table XXXVIII. this question is found fully answered. In fertile marriages generally there intervene about 17 months (1·38 year) between the ceremony and the birth of the first child. But in women of all ages this interval is far from being identical. As age increases above 25 years, the interval increases; the birth of a living child is longer deferred. The table does not confirm this statement for wives married at 40 and upwards; but this is almost certainly a mere result of the paucity of the data at these ages. The whole tenor of the table confirms the law of greatest fecundity according to age, meaning by fecundity likelihood of having children. For, it is observed, that not only are wives most fecund from 20 to 24, but also that they begin the career of fertility sooner than their younger or elder sisters.



TABLE XXXVIII.—SHOWING THE INTERVAL BETWEEN MARRIAGE AND THE BIRTH OF A FIRST CHILD IN WIVES MARRIED AT DIFFERENT AGES.

		MOTHER'S AGE AT MARRIAGE.							TOTAL.
		15-19.	20-24.	25-29.	30-34.	35-39.	40-44.	45-49.	
Years Married.	Less	94	325	126	44	15	4	...	608
	1	409	1259	533	135	49	3	2	2390
	2	83	202	88	45	17	2	...	437
	3	25	50	35	12	10	1	...	133
	4	8	31	13	8	1	...	...	61
	5	13	10	3	3	3	...	...	32
	6	5	14	6	1	1	...	...	27
	7	5	3	1	3	...	...	...	12
	8	1	3	1	...	...	...	...	5
	9	2	3	...	...	...	...	...	5
	10	...	1	...	...	...	...	...	1
	11	...	1	2	...	...	...	...	3
	12	2	1	1	...	...	...	...	4
	13	1	1	...	...	...	...	...	2
	14								
	15	1	...	...	...	...	...	...	1
	16								
	17								
	18	...	1	...	...	...	..	...	1
	Total	649	1905	809	251	96	10	2	3722
Average interval between Marriage and Birth of first Child.	Year.	1·516	1·329	1·350	1·510	1·594	1·400	1·000	1·385
	Months.	18·2	15·9	16·2	18·1	19·1	16·8	12·0	16·6

It is noteworthy, that while the average interval between marriage and the birth of the first child is seventeen months, the average interval between the births of successive children, however numerous, is a little under twenty months; the two intervals approxi-



mating one another so closely as to destroy all probability of the truth of the explanations usually offered for the delay of impregnation after a recent childbirth, and of the efficacy of continued lactation in retarding the occurrence of a new conception. And we shall soon see, in a quotation from Sadler, that he finds that women who do not suckle their offspring have as long an interval between conceptions as others. But, while Sadler by this demonstration destroys the only physiological foundation for his invective against the rich who do not suckle, he nevertheless proceeds enthusiastically, as if the dictum of physiologists were valid, even after their argument was ruined.

Speaking of the interval between marriage and a first birth, Sadler gives the following indefinite statement:—"Married females do not become fruitful, on the average, during the first year of their nuptials, but nearly so. A great number of cases which I have collected, with a view of determining this point, give three-fourths of them as producing their first child at the average of one year after marriage." \*

Whitehead,† founding on the observation of 541 married women of the average age of twenty-two years, makes out the average interval between marriage and the birth of a first child to be  $11\frac{1}{2}$  months.

Quetelet‡ admits, with sufficient probability, as an average term, that the birth of the first-born takes place

\* *The Law of Population*, vol ii. p. 30.

† *On Abortion and Sterility*, p. 242.

‡ *Treatise on Man*, p. 15.



within the first year which follows marriage. His error, as those of the others, depends upon the acknowledged want of documents.

TABLE XXXIX.—SHOWING THE AVERAGE DURATION OF MARRIAGE AT BIRTH OF EACH SUCCESSIVE CHILD ; AND THE AVERAGE INTERVAL BETWEEN THE BIRTHS OF THE SUCCESSIVE CHILDREN.\*

Number of Children.	Number of Mothers.	Duration of Marriage in Months.	Average interval between successive Births.
1	3722	17	17·0
2	2893	38	19·0
3	2534	64	21·3
4	1982	90	22·5
5	1543	115	23·0
6	1221	137	22·8
7	848	162	23·1
8	641	181	22·6
9	425	203	22·5
10	222	225	22·5
11	152	235	21·4
12	61	246	20·5
13	34	263	20·2
14	11	281	20·1
15	6	280	18·7
16	2	336	21·0
17	2	252	14·8
18	1	252	14·0
19	1	204	10·7
		Average	19·9

\* This is not a correct statement of the contents of this table. The last column does not directly give the average interval between the births of successive children, but the average interval between marriage and the birth of the child, divided by the number of the children born. For brevity's sake, the title is left as it stands.



It next comes to be inquired at what rate children succeed each other in families. This interesting topic is developed from the data given in Table XXXIX. It is formed by dividing the whole years of duration of sets of marriages, of different durations, by the number of children born in the corresponding marriages; and it must be remembered that as our data all spring from women who were fertile on the year of our census or counting, no women are included who, although fertile formerly, have now ceased to be so; and it is evident that, for the purposes of our argument, this is just.

The first conclusions deducible from the data are—

1. That the mass of early or first children, up to the third or fourth, come into the world in more quick succession than those that immediately follow.

2. That a mass of children, numbering from the fourth or fifth on to the tenth, succeed one another more slowly than those of the first category, and of the third.

3. That a mass of children, following the tenth, come into the world hurrying after one another with a gradually-increasing rapidity, which excels that of all their predecessors (a circumstance which may, in part at least, account for the great mortality of women bearing children after the ninth).\*

While all these propositions are true of a large number of children, it must not be supposed that they

\* *Edinburgh Medical Journal*, September 1865, p. 209; and Part VII. of this volume.



directly indicate laws regulating the fertility of women. But the table bears important information relative to this last topic. And it appears to me that the first of the three conclusions given above can be explained only by supposing what may therefore be held as equally well demonstrated—

(1.) That wives bearing their early children, up to the third or fourth, breed more rapidly than they subsequently do.

For the average fertility of all wives is at least 4 children ; and the great mass of fertile wives is therefore included in the calculation. All the wives destined to bear large families, and furnish data for the second and third conclusions, are included in the data for first 4 children. The mass of children born in families numbering 11 and more is not large enough to have great influence on the data, should it be the case that they are proportionately very quick breeders from the first.

If we now regard the mothers whose children have afforded the data for the second conclusion as to the rapidity of the succession of a mass of children, we shall have, I think, no difficulty in accepting the proposition—

(2.) That wives produce their children, numbering from the third or fourth on to the tenth, at greater intervals than their earlier progeny.

For, in the calculations, the earlier and more rapidly-succeeding progeny are included, and have their full influence, and diminish the periods given in



the table opposite children numbering from 4 to 10, reducing them below what they would be were pregnancies from 4 to 10 alone counted, exclusive of those from 1 to 4.

Regarding now the mothers of families numbering 11 or more, it is evident that their paucity, though not such as to destroy all their value, is such as to prevent their having a paramount influence upon the figures of the two preceding categories. It might therefore appear necessary to leave undecided whether their specially rapid bearing were a consequence of their great fertility, and therefore an acquired or secondary rapidity, or were an original condition true of even their earlier pregnancies. That the latter is to be accepted to the exclusion of the former supposition is evident, if we observe that the married life of the women with families above 10 is not long enough to admit of their having gone through the series of lengths of pregnancies given in the table opposite each successive child. It is thus shown—

(3.) That wives bearing more than 10 children, or wives bearing very large families, breed more rapidly than others during their whole child-bearing lives.

Wives, therefore, who bear numerous progeny, do so in virtue of two differences from other women. They bear their children more rapidly, and they continue fertile longer than their neighbours.

Were the third conclusion just given not before us, it might be supposed that the rapid bearing of earlier children was a result of youth and vigour. This sup-



position is not only inconsistent with the third conclusion, but with the law to be hereafter demonstrated, that the oldest women, who are continuedly fertile, bear children more rapidly than any other.

The average length of interval between all successive children is (19·9) nearly 20 months.

I have frequently heard it said that a fertile woman bears a child every two years. Some authors have made careful statements on this point. Whitehead\* says that fertile women produce children every 20 months; but "this includes abortions, false conceptions, so-called premature deliveries, and all having an unsuccessful issue, the average amount of which will be rather more than  $1\frac{1}{2}$  for each individual." Sir William Petty long ago laid it down that "every teeming woman can bear a child once in two years." Malthus† adopts the same period, and refers to the *Statistical Account of Scotland* as confirming it. The number and exactness, however, of the data here adduced, and the circumstance that they include only children born alive (excluding still-born and abortions), leave no room for doubt that all the authors referred to under-estimate the rate at which married women bring children into the world.‡

\* *On Abortion and Sterility*, p. 245.

† *An Essay on the Principle of Population*, vol. ii. p. 3.

‡ See also Robertson's *Essays and Notes on the Physiology of Women*, p. 185. His conclusions (p. 193) are as follows:—

"The first corollary which I would draw from the facts collected in Manchester and in York, is, that in 7 out of 8 women who suckle



On this point Sadler is so full and distinct that I quote his words :—"The interval of time," says he, "at which the fruitful couples produce their children, calculated from the period of their marriage to the birth of their last child, including the greater prolificness of the first year, exceeds 2 years. It extends to between  $2\frac{1}{4}$  and  $2\frac{1}{2}$  years, if calculated from the first birth."\* In this calculation, as in that of the interval between marriage and the birth of a first child, Sadler evidently errs, making the former too long and the latter too short. For both he gives no data ; yet, in regard to the interval between the births of successive children he says :—"All the tables are constructed upon the presumption of its certainty, and happily it is one which, on this very debatable question, has never been made the subject of controversy, and which does not admit of it. Nothing," he continues, "is more certain or better ascertained than the average period at which the human female, in a state of prolificness, reprodur as long a period as the working classes in this country are in the habit of doing, there will elapse an interval of from 12 to 15 months from parturition to the commencement of the subsequent pregnancy.

"*Second*, That in a majority of instances, when suckling is prolonged to even 19 or 20 months, pregnancy does not take place till after weaning.

"*Third*, That lactation having this influence on the generative function, we are warranted in regarding the secretion of milk as the cause which regulates the periods of conception in mankind, as instinct operates to the same end in graminivorous quadrupeds, and probably in all other animals."

\* Vol. ii. p. 30.



duces. Were we, indeed, to form our general rules from particular exceptions, we should in this, as in all other cases, be grievously misled. We might conclude, for instance, that she would continue to multiply within the year; but general computations will rectify any such error, and conduct us to conclusions which are not only reconcilable with philosophy and truth, but resolvable into the ordinations of a merciful Providence. The human mother has to feed her infant for a period pretty nearly corresponding in length to that of gestation (I speak now as regards the necessity of the great mass of the community, with whom the question evidently rests); nature, therefore, has kindly ordained, as a general rule, that the period of impregnation shall be postponed till that essential duty is discharged, and for a period somewhat beyond it; and he must be ignorant indeed who does not see most clearly that the health, and indeed frequently the existence, of both mother and offspring are secured by this physical regulation of the common parent of mankind. The human being, in reference to the term of existence, multiplies later, and at longer intervals, and ceases to be prolific sooner, than any other animated being with whom we are acquainted; hence we find, on the average, that in the maternal state, during its period of fruitfulness, the births are not so frequent as once in two years. Even in the rank of society which is absolved from the necessity (though not from the duty) of fulfilling one of the most important of the maternal offices, that of feeding from their own bosoms their infant offspring, and who



too often avail themselves of that unnatural immunity, consequently removing what our physiologists regard as one of the physical impediments to an accelerated prolificness,\*—even in this rank, I find the births are at intervals of about, but rather exceeding, two years. That period, therefore, as it respects the mass of the community, who are differently circumstanced in this respect, cannot be shorter. But arguments and proofs on this point are unnecessary, no writer having ever ventured upon supposing a shorter period than two years possible ; and even Sir William Petty, when labouring to prove the possibility of a doubling every ten years for a century after the flood, amongst his other suppositions, so extravagant if applied to the present era, only lays it down that every teeming woman can bear a child ‘ once in two years.’”

\* On this subject the work of Robertson already cited may be consulted ; also a paper by Professor Laycock, quoted by Robertson.



## CHAPTER XI.

FERTILITY OF WIVES-MOTHERS MARRIED AT  
DIFFERENT AGES.

BEFORE discussing this and the next topics, it is necessary to remark that fertility may be maintained in degree in two ways—either by long continuance or by intensity while it lasts. At present I omit entirely the consideration of the intensity of fertility while it lasts, taking up this in the next part. But I shall show that, of a mass of fertile women, the younger are on the whole more fertile than the older. To demonstrate this I first adduce a table drawn from the data of

TABLE XL.—SHOWING THE FERTILITY OF WIVES-MOTHERS MARRIED AT DIFFERENT AGES, FROM THE DATA OF ST. GEORGE'S-IN-THE-EAST.

Mother's Age at Marriage.	$11\frac{5}{12}$ Years Married. —— Average Number of Children.	$21\frac{5}{12}$ Years Married. —— Average Number of Children.
15-19	5·0	7·7
20-24	4·5	7·0
25-29	4·4	6·4
30-34	3·4	3·0

St. George's-in-the-East. It is evident here that the



younger women 11 years married, and also those 21 years married, have on an average larger families than the elder, of whatever respective ages. It must be observed that the table includes all wives who, in a small selected population, have shown any fertility; and it must be added that the committee of the Statistical Society have enunciated the same conclusion. I quote their own words:—"The following abstract will show the average number of children to each marriage, at the respective periods of 10, 20, 30, and 40 years after the birth of the first child, for each class of marriages formed at the four different quinquennial periods of life :—

TABLE XLI.

Years elapsed since Birth of First Child.	Average number of Children to each Marriage formed at Ages			
	16-20	21-25	26-30	31-35
10	5·05	4·51	4·42	3·44
20	7·68	7·01	6·43	3·00
30	8·41	7·89	6·80	7·00
40	10·85	8·24	5·00	4·00

"It is thus obvious that marriages formed under the age of twenty-five are more prolific than those formed after that age, and that those formed between sixteen and twenty years of age are still more so than those at any of the superior ages."\*

\* *Journal of the Statistical Society of London*, vol. xi. p. 223.



As doctrine is still taught exactly the opposite of that here sustained, it is important to establish the latter, if possible, by further proof. At another place I shall show the erroneous interpretation of the data which have been adduced in support of the opposite doctrine—namely, that marriages formed late in life are more prolific than those formed earlier.

The figures now to be adduced not only confirm the doctrine that early marriages are more fruitful than late marriages ; they also explain it, showing that the younger married have a longer continuance of fertility than the older married, allowing to both the same duration of marriage, and all within the child-bearing period of life. So far as the demonstration has hitherto gone, we have shown that the younger are more fertile than the elder ; that, excluding those who have no children, the younger will bear larger families than the elder. We have not shown which bear their children most rapidly—that is, which have the greatest intensity of fertility while it lasts—leaving this topic for another chapter. We now proceed to show that, among the fertile, the younger have a longer continuance of fertility than the elder. It is this last circumstance which accounts for the greater fertility of the marriages of the younger. The following table demonstrates this. It needs no explanation. The details are given in the footnote.\*

\* The Table XLII. may be easily seen to be made up from the following five tables—XLIII. XLIV. XLV. XLVI. XLVII. In



TABLE XLII.—SHOWING THE AMOUNT OF CONTINUANCE IN FERTILITY OF WIVES MARRIED  
AT VARIOUS AGES (AS SHOWN WITHIN TWELVE MONTHS).

Age of Mother at Marriage	15-19	20-24	25-29	30-34	35-39	Total.
The number Child-bearing in the 5th year } of Married Life is 1 in . . . . . }	2.6	2.7	4.1	4.9	10.5	3.2
The number Child-bearing in the 10th year } of Married Life is 1 in . . . . . }	3.2	4.0	5.9	8.7	...	4.4
The number Child-bearing in the 15th year } of Married Life is 1 in . . . . . }	4.6	6.8	18.2	37.4	...	8.0
The number Child-bearing in the 20th year } of Married Life is 1 in . . . . . }	8.5	14.6	129.8	...	...	16.3
The number Child-bearing in the 25th year } of Married Life is 1 in . . . . . }	68.0	480.5	...	...	...	171.0

these five tables of the fertility of married life at different epochs, the number of wives-mothers at the respective epochs is the actual registered number in Edinburgh and Glasgow in 1855. The number of wives of different ages is got by estimating, and the Carlisle



In order to derive from Table XLII. more information as to the relative numerical value of the fertility of a mass of wives in the fifth, tenth, and fifteenth years of married life, and so on, I have framed the following Table (LIV.) I have freely pointed out

Table of Mortality is used. The estimate is not made in the exactest way, but the errors will not injure the comparison of the figures with one another, as the same (perhaps unavoidable) error is introduced into all. The results probably give a near approach to the true degrees of fertility; for, while among the child-bearing there are some omitted, there are probably fewer marriages omitted, and the number of wives as estimated would be too large were not a very high percentage taken off (1 in 100) for the special mortality of first confinements. (See Part VII. of this volume, and Dr. Stark's Report in the *Seventh Annual Report of the Registrar-General for Scotland*, p. xxxii.)

To find how many women, 5, 10, and 15 years married, are alive and not widowed in 1855, it would strictly be necessary to have the numbers married in 1850, 1845, and 1840, from which the estimates should be made. Instead of doing this, I have estimated from the number married in 1855. As the population is increasing not greatly, this error thus introduced will not be great.

It is partly with a view to correct this error that I have taken off an extravagantly high percentage for the mortality of first labours.

In making the estimate I have doubled the mortality in order to exclude the widowed.

TABLE XLIII.—FERTILITY OF WIVES IN THE FIFTH YEAR OF MARRIED LIFE.

Ages at Child-bearing .....	20-24	25-29	30-34	35-39	40-44	Total.
Number of Wives .....	644	1686	1008	358	179	3875
Number of Wives-Mothers .....	247	611	244	72	17	1191
Number Child-bearing, 1 in.....	2·6	2·7	4·1	4·9	10·5	3·2



the sources of error in the fundamental figures of Table XLII. ; and after all I flatter myself that in these fundamental figures there is an approach to truth such as to justify the further deduction of Table LIV. ; only it is necessary to mention that in this table there

TABLE XLIV.—FERTILITY OF WIVES IN THE TENTH YEAR OF MARRIED LIFE.

Ages at Child-bearing .....	25-29	30-34	35-39	40-44	Total.
Number of Wives .....	594	1528	902	313	3337
Number of Wives-Mothers .....	186	381	153	36	756
Number Child-bearing, 1 in.....	3·2	4·0	5·9	8·7	4·4

TABLE XLV.—FERTILITY OF WIVES IN THE FIFTEENTH YEAR OF MARRIED LIFE.

Ages at Child-bearing .....	30-34	35-39	40-44	45-49	Total.
Number of Wives .....	532	1360	782	262	2936
Number of Wives-Mothers .....	116	200	43	7	366
Number Child-bearing, 1 in .....	4·6	6·8	18·2	37·4	8·0

TABLE XLVI.—FERTILITY OF WIVES IN THE TWENTIETH YEAR OF MARRIED LIFE.

Ages at Child-bearing .....	35-39	40-44	45-49	Total.
Number of Wives .....	477	1171	649	2297
Number of Wives-Mothers .....	56	80	5	141
Number Child-bearing, 1 in .....	8·5	14·6	129·8	16·3

TABLE XLVII.—FERTILITY OF WIVES IN THE TWENTY-FIFTH YEAR OF MARRIED LIFE.

Ages at Child-bearing .....	40-44	45-49	Total.
Number of Wives .....	408	961	1396
Number of Wives-Mothers.....	6	2	8
Number of Child-bearing, 1 in .....	68·0	480·5	171·0



are no actual values to keep it close to the truth. Taking, then, Table XLII. as giving actual values, we have the fertilities for 1855 ; or for 12 months. But as 20 months has been shown to be the average time-

I have now to add six tables, XLVIII. to LIII. inclusive. These are constructed with a view to meet what might form a reasonable criticism on the six preceding, XLII. to XLVII. inclusive. In these latter are included all the married—that is, the fertile and sterile. Now, the amount of sterility varies in marriages at different ages, and it may be considered desirable to eliminate this source of difference in order to have a view of the duration of fertility in those married women who are fertile. The tables of sterility hereafter given afford means of estimating the proportion sterile in marriages at different ages. By this means the tables XLVIII. to LIII. are constructed. They give a view of the duration of the fertility of fertile women married at different ages.

TABLE XLVIII.—SHOWING THE DURATION OF FERTILITY IN FERTILE WIVES MARRIED AT VARIOUS AGES (AS SHOWN WITHIN TWELVE MONTHS).

Age of Mother at Marriage ....	15-19	20-24	25-39	30-34	35-39	Total.
The number Child-bearing in the 5th year of Married Life is 1 in .....	2·4	2·7	3·0	3·1	4·9	2·8
The number Child-bearing in the 10th year of Married Life is 1 in .....	3·0	4·0	4·2	5·4	...	3·9
The number Child-bearing in the 15th year of Married Life is 1 in ..	4·2	6·8	13·1	23·3	...	7·0
The number Child-bearing in the 20th year of Married Life is 1 in .....	7·9	14·6	95·8	...	...	14·7
The number Child-bearing in the 25th year of Married Life is 1 in .....	63·0	480·5	...	...	...	167·4



unit of fertility, the fertilities of 1855 must be increased in like proportion ; for as 12 is to 20 so are the fertilities given in Table XLII. to the real fertilities. All the fertile women cannot be presumed to have shown that quality in 12 months, but all may be presumed to have shown it in 20 months. In this way

TABLE XLIX.—FERTILITY OF FERTILE WIVES IN FIFTH YEAR OF MARRIED LIFE.

Ages at Child-bearing .....	20-24	25-29	30-34	35-39	40-44	Total.
Number of Wives .....	644	1686	1008	358	179	3875
Number of Wives-Mothers .....	597	1686	728	223	84	3318
Number Child-bearing in 5th } year of Married Life .....	247	611	244	72	17	1191
Or of Wives-Mothers, 1 in .....	2·4	2·7	3·0	3·1	4·9	2·8

TABLE L.—FERTILITY OF FERTILE WIVES IN TENTH YEAR OF MARRIED LIFE.

Ages at Child-bearing .....	25-29	30-34	35-39	40-44	Total.
Number of Wives .....	594	1528	902	313	3337
Number of Wives-Mothers .....	551	1528	650	195	2924
Number Child-bearing in 10th year of } Married Life .....	186	381	153	36	756
Or of Wives-Mothers, 1 in .....	3·0	4·0	4·2	5·4	3·9

TABLE LI.—FERTILITY OF FERTILE WIVES IN FIFTEENTH YEAR OF MARRIED LIFE.

Ages at Child-bearing .....	30-34	35-39	40-44	45-49	Total.
Number of Wives .....	532	1360	782	262	2936
Number of Wives-Mothers .....	493	1360	565	163	2581
Number Child-bearing in 15th year of } Married Life .....	116	200	43	7	366
Or of Wives-Mothers, 1 in .....	4·2	6·8	13·1	23·3	7·0



the following table (LIV.) may be held as an estimate of the comparative amount of fertility in living children, shown by wives at different epochs of married life.

The table shows a gradually-diminishing amount of perseverance in fertility as age advances. In illustration of the mode of reading it, I may state that about a half of all wives are fertile at the fifth year of married life ; more than a third are fertile at the tenth year of married life ; and only a fifth part of the whole wives arrived at the fifteenth year of married life are fertile ; and so on.

Another interesting result is got from this Table (LIV.), by comparing the different horizontal columns with one another. Reading the figures of adjacent columns obliquely from below upwards, we have a

TABLE LII.—FERTILITY OF FERTILE WIVES IN TWENTIETH YEAR OF MARRIED LIFE.

Ages at Child-bearing .. .. .	35-39	40-44	45-49	Total.
Number of Wives .. .. .	477	1171	619	2297
Number of Wives-Mothers .. .. .	442	1171	469	2082
Number Child-bearing in 20th year of Married Life .. .. .	56	80	5	141
Or of Wives-Mothers, 1 in. ....	7.9	14.6	95.8	14.7

TABLE LIII.—FERTILITY OF FERTILE WIVES IN TWENTY-FIFTH YEAR OF MARRIED LIFE.

Ages at Child-bearing .. .. .	40-44	45-49	Total.
Number of Wives .. .. .	408	961	1396
Number of Wives-Mothers .. .. .	378	961	1339
Number Child-bearing in 25th year of Married Life ...	6	2	8
Or of Wives-Mothers, 1 in .. .. .	63.0	480.5	167.4



comparison of the fertility of a mass of wives of the same age, but of quinquennial differences of duration of marriage. And it is very interesting to observe that the younger married closely approach in fertility those married five years later in life, both being arrived at the same year of life at the time of the comparison.

Short and Sussmilch maintain that early marriages are not favourable to the population. But, so far as I know, they adduce no satisfactory evidence whatever for their belief. Yet they have considerable authority on their side, including the redoubtable Sadler, who arrays in his support the venerable names of Aristotle, of Plato, of Virgil, and of Plutarch.

It is to be remarked, that I here object to this statement of these authors only so far as the number of living births is concerned, and I do not consider the diminished chances of survival which children of very early marriages are believed to have. There can be, in my opinion, no doubt that early marriages are most favourable to the population;\* and as I have already shown that wives under twenty are less fecund than those from twenty on to at least twenty-four years of age,† the fertility of the younger, as a mass, is the more striking. But although most highly fertile as a mass, the number of sterile among those married under twenty years of age is not inconsiderable, and it is probably this amount of sterility which, while

\* See Chapter XIV. of this Part, farther on.

† *Transactions of the Royal Society*, 1864, or Table XIV. in this volume, p. 35.



satisfactory statistical evidence was deficient, has given rise to the error now commented upon. The authors referred to give no definition of what they mean by

TABLE LIV.—SHOWING THE PROBABLE AMOUNT OF CONTINUANCE IN FERTILITY, AT DIFFERENT EPOCHS, OF A MASS OF WIVES MARRIED AT VARIOUS AGES.\* (See next page.)

Age of Mother at Marriage . . . . .	15-19	20-24	25-29	30-34	35-39	Total.
The proportion Child-bearing about the 5th } year of Married Life is 1 in . . . . .	1.56	1.62	2.46	2.94	6.30	1.92
	64.1	61.7	40.6	34.0	15.9	52.1
Or a percentage of . . . . .						
The proportion Child-bearing about the 10th } year of Married Life is 1 in . . . . .	1.92	2.40	3.54	5.22	...	2.64
	52.1	41.7	28.2	19.2	...	37.9
Or a percentage of . . . . .						
The proportion Child-bearing about the 15th } year of Married Life is 1 in . . . . .	2.76	4.08	10.92	22.44	...	4.80
	36.2	24.5	9.1	4.5	...	20.8
Or a percentage of . . . . .						
The proportion Child-bearing about the 20th } year of Married Life is about 1 in . . . . .	5.10	8.76	77.88	...	...	9.78
	19.6	11.4	1.3	...	...	10.2
Or a percentage of . . . . .						
The proportion Child-bearing about the 25th } year of Married Life is about 1 in . . . . .	40.80	288.3	...	...	...	102.6
	2.4	.35	...	...	...	.97
Or a percentage of . . . . .						



early marriage. Whatever they may mean, they have no good evidence for their doctrine.

Quetelet† enunciates on this topic the following doctrine as a natural consequence from his data and reasonings. A marriage, says he, if it be not barren, produces the same number of births at whatever period it takes place, provided the age of the woman does not exceed twenty-six years. After this age the number of children, he adds, diminishes. Not only do I, of course, think Quetelet wrong in his conclusions, but I

\* I here subjoin a table identical with the preceding (LIV.), except that it is corrected for sterility, just as Tables XLVIII. to LIII. inclusive have been.

TABLE LV.—SHOWING THE PROBABLE AMOUNT OF CONTINUANCE IN FERTILITY AT DIFFERENT EPOCHS OF FERTILE WIVES MARRIED AT VARIOUS AGES.

Age of Mother at Marriage .	15-19	20-24	25-29	30-34	35-39	Total.
The proportion Child-bearing about the 5th year of Mar- ried Life is 1 in . . . . }	1·44	1·62	1·80	1·86	2·94	1·68
Or a percentage of . . . .	69·4	61·7	55·5	53·8	34·0	59·4
The proportion Child-bearing about the 10th year of Mar- ried Life is 1 in . . . . }	1·80	2·40	2·52	3·24	...	2·34
Or a percentage of . . . .	55·5	41·7	39·7	30·8	...	42·7
The proportion Child-bearing about the 15th year of Mar- ried Life is 1 in . . . . }	2·52	4·08	7·86	13·98	...	4·20
Or a percentage of . . . .	39·7	24·5	12·7	7·1	...	23·8
The proportion Child-bearing about the 20th year of Mar- ried Life is 1 in . . . . }	4·74	8·76	57·48	...	...	8·82
Or a percentage of . . . .	21·1	11·4	1·7	...	...	11·3
The proportion Child-bearing about the 25th year of Mar- ried Life is 1 in . . . . }	37·80	288·30	...	...	...	100·44
Or a percentage of . . . .	2·6	·38	...	...	...	·99

† *Treatise on Man*, p. 15.



cannot in his work discover any satisfactory grounds for them.

Before passing from the perseverance in fertility of the early married, I shall point out a difficulty of which it gives the solution. In Part I. Chapter V. I showed that fecundity in wives from fifteen to nineteen years of age is less than at from twenty to twenty-four; that is, of the young women fewer have children. At the same time (Chap. III.) I showed that the fecundity of the mass of wives in our population is greatest at the commencement of the child-bearing period of life, and after that epoch gradually diminishes; that is, those not the most fecund do, as a mass, produce most children. These two propositions are, at first sight, difficult to reconcile; and it is accordingly satisfactory to be able to show that the greater continuance in fertility of the mass of younger wives is the explanation of the apparent anomaly. To illustrate how the tables read in affording this explanation, I may state that while I formerly showed that the wives from fifteen to nineteen years of age are not so fecund as those from twenty to twenty-four years of age, the tables last adduced show that at the 5th year of marriage the youngest married—that is, at ages from fifteen to nineteen—already surpass all others in fertility, 1 in 1·44 bearing; that at the tenth year of marriage they still further surpass in fertility all others, 1 in 1·80 bearing; and that at the fifteenth year of marriage they in a still higher degree surpass all others, 1 in every 2·52 bearing children within a year. [Table LV.]



Finally, under this head I notice an important element of the inexactness that enters into the data here used—namely, the occurrence of second and third marriages. But the influence of this element is almost certainly inconsiderable, for the following reasons :—In cases of second and subsequent marriages the data used are exclusively those of the last marriage ; as far as is known, a woman's previous marriage does not interfere with her subsequent fertility ; it is shown in this paper that a woman's previous fertility tends to ensure continuance in fertility ; it will be shown that a woman's previous fertility tends to diminish the intensity of her subsequent fertility, when that is compared with the fertility of women late in being married and having family ; and the admixture of second and subsequent marriages in the data which include only the last marriage, would tend to diminish the force of the results as bearing out these conclusions. They are therefore all the more secure, from the fact of the intermingling of some data which would diminish their apparent influence.

Another element of inexactness I shall only mention—the occurrence of twins, and both being counted in the figures.



## CHAPTER XII.

FERTILITY OF PERSISTENTLY FERTILE WIVES OF  
DIFFERENT AGES.

I MAY here repeat, that by persistently fertile I mean fertile at, or up till, the time of the collection of the data ; and I adduce a table which clearly shows, so far as the mass of figures can be relied on, that the fertility of the elder is greater than of the younger, while it lasts ; or, in other words, the fertility of the elder is the more intense.

Table LVI. is read in the following manner :—To take the second line—Fertile women five years married, and under ten, have, if they are now from 15 to 19 years of age, 2·5 children ; if now from 20 to 24 years of age, 3·19 children ; if now from 25 to 29 years of age, 3·75 children ; and so on.



TABLE LVI.—SHOWING THE INTENSITY OF FERTILITY IN WIVES-MOTHERS OF DIFFERENT AGES.

DURATION OF MARRIAGE.	MOTHER'S AGE.						
	15-19.	20-24.	25-29.	30-34.	35-39.	40-44.	45-49.
Under 5 years	1·128	1·519	1·825	1·844	1·827	1·698	1·200
5 years and under 10	2·500	3·190	3·750	4·048	4·085	3·792	4·000
10 years and under 15	...	5·333	5·453	5·903	6·197	5·964	6·500
15 years and under 20	...	...	6·000	...	7·914	7·993	8·435
20 years and under 25	...	...	...	7·000	9·396	9·718	10·528
25 years and under 30	...	...	...	...	...	12·368	13·600
30 years.	...	...	...	...	...	...	13·000



The conclusion here arrived at is founded upon lengths of married life.\* Were the figures such as to give, instead of lengths of married life, length of intervals between the births of first and last children, the results would be still more striking ; for I have already shown that, in the case of the elder, fertility is later in beginning to show itself than in the younger.

\* The following tables give all the details and calculations from which Table LVI. is constructed :—

TABLE LVII.—OF WOMEN UNDER 5 YEARS MARRIED.

	No. of Mothers.	No. of Children.	Average to each Mother.
	7183	11,880	1·654
Mother's age—16 to 19 years. .	374	422	1·128
„ 20 to 24 „ . .	3180	4829	1·519
„ 25 to 29 „ . .	2460	4489	1·825
„ 30 to 34 „ . .	833	1536	1·844
„ 35 to 39 „ . .	277	506	1·827
„ 40 to 44 „ . .	53	90	1·698
„ 45 to 49 „ . .	5	6	1·200

TABLE LVIII.—OF WOMEN 5 YEARS MARRIED AND LESS THAN 10.

	No. of Mothers.	No. of Children.	Average to each Mother.
	4637	17,690	3·815
Mother's age—16 to 19 years . .	2	5	2·500
„ 20 to 24 „ . .	499	1592	3·190
„ 25 to 29 „ . .	2155	8082	3·750
„ 30 to 34 „ . .	1418	5740	4·048
„ 35 to 39 „ . .	461	1883	4·085
„ 40 to 44 „ . .	96	364	3·792
„ 45 to 49 „ . .	5	20	4·000



If, as I have shown, the younger are more prolific than the elder, and if, as I also have shown, the elder are more intensely fertile while their fertility lasts, than the younger in the same time; then it necessarily follows, as a corollary, that the fertile women married younger have a longer continued fertility than the fertile women married older. In no other way can the younger surpass the elder in their whole fertility; a conclusion which has already been otherwise demonstrated.

It may also be here pointed out that the figures of

TABLE LIX.—OF WOMEN 10 YEARS MARRIED AND LESS THAN 15.

	No. of Mothers.	No. of Children.	Average to each Mother.
	2739	16,233	5.930
Mother's age—20 to 24 years . .	9	48	5.333
„ 25 to 29 „ . .	415	2263	5.453
„ 30 to 34 „ . .	1345	7939	5.903
„ 35 to 39 „ . .	814	5044	6.197
„ 40 to 44 „ . .	140	835	5.964
„ 45 to 49 „ . .	16	104	6.500

TABLE LX.—OF WOMEN 15 YEARS MARRIED AND LESS THAN 20.

	No. of Mothers.	No. of Children.	Average to each Mother.
	1280	10,013	7.823
Mother's age—25 to 29 years . .	7	42	6.000
„ 30 to 34 „ . .	253	1867	7.379
„ 35 to 39 „ . .	721	5706	7.914
„ 40 to 44 „ . .	273	2182	7.993
„ 45 to 49 „ . .	23	194	8.435



Table XXXIX. make it probable that elderly women when fertile are more intensely so than younger, when their fertility has already resulted in a large family, for that table shows that the children in large families are born very quickly one after another.

In his work on *The Law of Population*, Mr. Sadler enters upon this subject of the varying fertility

TABLE LXI.—OF WOMEN 20 YEARS MARRIED AND LESS THAN 25.

	No. of Mothers.	No. of Children.	Average to each Mother.
	432	4181	9·678
Mother's age—30 to 34 years . .	1	7	7·000
„ 35 to 39 „ . .	134	1259	9·396
„ 40 to 44 „ . .	259	2517	9·718
„ 45 to 49 „ . .	36	379	10·528

TABLE LXII.—OF WOMEN 25 YEARS MARRIED AND LESS THAN 30.

	No. of Mothers.	No. of Children.	Average to each Mother.
	29	371	12·793
Mother's age—40 to 44 years . .	19	235	12·368
„ 45 to 49 „ . .	10	136	13·600

TABLE LXIII.—OF WOMEN 30 YEARS MARRIED.

	No. of Mothers.	No. of Children.	Average to each Mother.
	1	13	13·000
Mother's age—45 to 49 years . .	1	13	13·000



of women according to age. Seeking arguments wherewith to overturn the teaching of Malthus, whose principles he hated as well as opposed, he found data which at first sight appear to support his doctrine "that marriages are more prolific the longer they are deferred." Were this true doctrine, it would certainly go far to overturn the Malthusian system, and Mr. Sadler might be justly proud of the demonstration. The facts which he adduces may, without cavil, be allowed to be, as he says, indisputable. It is his illogical use of the facts which has to be pointed out. Without pretending to enter on the defence of Malthusian notions, we accept Mr. Sadler's challenge "to evade the demonstration" which the aforesaid facts afford. And it is of importance to do so, because, down to the latest authors, Sadler's facts and supposed demonstrations are quoted with unsuspecting approval.\*

The first data afforded by Sadler are derived from the records of Dr. Granville's experience as physician to the Benevolent Lying-in Institution and the Westminster Dispensary, the calculations having been made by Mr. Finlayson.

\* See Boudin, *Traité de Géographie et de Statistique Médicales*, etc., tome ii. p. 59.



TABLE LXIV.—SHOWING THE EFFECT THE POSTPONEMENT OF THE MARRIAGES OF FEMALES HAS UPON THEIR ANNUAL PROLIFICNESS. (SADLER.)

Age when Married.	Average number of Births for each year of Marriage.
From 13 to 16 . . .	·456706
„ 16 to 20 . . .	·503610
„ 21 to 24 . . .	·520227
„ 25 to 28 . . .	·545163
„ 29 to 32 . . .	·589811
„ 33 to 36 . . .	·776866
„ 37 to 39 . . .	1·125000

Now, this table is made from the data of lying-in charities. It is therefore not a table of fertile women, but of persistently fertile women; for every woman was entered in the records only when she came to have attendance in her confinement. All that the table offers is corroboration of the law enunciated in this chapter, that elderly women are more fertile than younger so long as their fertility endures.

It is almost incredible that so acute a reasoner as Mr. Sadler is, could be so deceived by appearances as to suppose his figures showed that marriages at thirty-nine years of age are as fruitful as marriages of any age down to thirteen. Yet, for aught he says, he appears so to believe.

Sadler did indeed get the length of seeing that the table just given was somewhat deficient. “It may,”



he says,\* “perhaps be objected to the whole of the foregoing proofs, that they are derived from a register which cannot profess to give the whole number of children which the marriages it records shall produce from their commencement to their termination, but only those which have been born to each up to a period within these limits, all the facts which it can record being necessarily retrospective ones. I shall therefore,” he continues, “proceed to another series of proofs of the same principle, which will at once silence every such exception, and afford a strong additional demonstration of its truth. These are derived from the registers of the peerage, which, as I have observed elsewhere, I have gone through in order to collect a body of authentic facts illustrative of many of the principles advanced in these volumes. As far as they relate to the subject before us, those facts are as follows:”—

TABLE LXV.—SHOWING THE EFFECT OF THE POSTPONEMENT OF THE MARRIAGES OF THE PEERESSES ON THEIR PROLIFICNESS. (SADLER.)

Period of Marriage.	Number of Marriages.	Number of Children.	Births to each Marriage.
From 12 to 15 . . .	32	141	4.40
„ 16 to 19 . . .	172	797	4.63
„ 20 to 23 . . .	198	1033	5.21
„ 24 to 27 . . .	86	467	5.43

\* *Law of Population*, vol. ii. p. 279.



To this table of Sadler's many objections may be made, such as the paucity and insecurity of the data, as also their deficiency, the highest age of marriage included in them being only twenty-seven, and all notice of the important element of the duration of marriage being omitted.

Sadler not only erred in supposing he had demonstrated that late marriages are more prolific than early; he was ignorant also that a larger proportion of the elder than of the younger wives bears no children at all, and that an older woman continues fertile a shorter time than a younger, counting, in both cases, only up to periods within the child-bearing portion of life.

It is a natural, and I believe a true notion, that twin-bearing is a sign of intense fertility in woman, as the number of a litter certainly is in bitches and other inferior animals. In confirmation of this notion, and of the law of intensity of fertility now demonstrated, we find that women are more likely to bear twins the older they are. This subject is capable of some interesting developments; but, as I have already elsewhere\* entered upon them, I shall add no more in this place.

In like manner, it is natural to suppose that the length and weight of children should go with intensity of fertility. Yet my researches† seem to show that this is not the case, but that length and weight of

\* *Edinburgh Medical Journal* for March and April 1865; and Part III. of this volume.

† *Edinburgh Medical Journal* for December 1864, and Part II. of this volume.



children go with the intensity of fecundity, or likelihood of bearing children, according to age. Professor Hecker, of Munich, has, however, elaborately shown that my conclusions on this head do not agree with those derived from his larger data.\* Mine are based on 2087 observations only, and I am willing, in the meantime, to hold it as *sub judice*, whether his or my conclusions are to be received. His do appear to me to be more probable because they bring the laws of length and weight of children, according to the mother's age, into agreement with the law of intensity of fertility here demonstrated.

\* *Monatsschrift für Geburtskunde und Frauenkrankheiten*, November 1865.



## CHAPTER XIII.

## THE FERTILITY OF THE OLDER WOMEN.

So ardently did Sadler desire the triumph of his attack on Malthus, that he adopted the dream of Mason Good, who says "that the usual term (of cessation of the menses) is between forty and fifty, except where women marry late in life, in which case, from the postponement of the generative orgasm, they will occasionally breed beyond their fiftieth year"!!\* Mason Good refers to some extraordinary cases of pregnancy in old women, curiosities in physiology, but he adduces no good evidence in favour of the doctrine he here propounds. An opposite doctrine is taught by Burns, an author equally celebrated, and much more worthy of confidence in a question of the kind now before us. "It is well known," says the Glasgow professor,† "that women can only bear children until a certain age, after which the uterus is no longer capable of performing the action of gestation, or of performing it properly. Now it is observable that this incapability or imperfection takes place sooner in those who are advanced

\* *The Study of Medicine*, 1822, vol. iv. p. 63.

† *Principles of Midwifery*, tenth edition, p. 309.



in life before they marry, than in those who have married and begun to bear children earlier. Thus we find that a woman who marries at forty shall be very apt to miscarry, whereas, had she married at thirty, she might have borne children when older than forty; from which it may be inferred that the organs of generation lose their power of acting properly sooner, if not employed, than in the connubial state. The same cause which tends to induce abortion at a certain age, in those who have remained until that time single, will also, at a period somewhat later, induce it in those who have been younger married; for in them we find that, after bearing several children, it is not uncommon to conclude with an abortion; or, sometimes after this incomplete action, the uterus, in a considerable time, recruits, as it were, and the woman carries a child to the full time, after which she ceases to conceive." My own opinion has always coincided with that expressed by Burns;\* and I may add, that the curious observation regarding abortion at the close of the fertile period of life has its analogue in the lower animals. Several times I have been told by men of experience that an old bitch often ends her career of breeding by a dead and premature pup. Whitehead also† regards those pregnancies which occur near the termination of the fruitful period in

\* Burns' statement does not bear minute criticism, for he does not distinguish between sooner as implying earlier advent of relative sterility, and sooner as implying shorter duration of fertility.

† *On Abortion and Sterility*, p. 247.



women as being among the most commonly unsuccessful. More recently Dr. Arthur Mitchell has connected the occurrence of idiocy in a child with the circumstance of its being the last-born of its mother.\*

In Edinburgh and Glasgow, in 1855, 53 women above the age of 45 bore living children. Among those 53, only 1 was primiparous—her age was 49, and she had only been one year married; 2 bore second children—1 was aged 46 years, and had been four years married—the other was aged 52 years, and had been three years married; 4 bore fourth children; 4 bore fifth children; 3 bore sixth children; 3 bore seventh children; 6 bore eighth children; 8 bore ninth children; 7 bore tenth children; 4 bore eleventh children; 1 bore a twelfth child; 4 bore thirteenth children; 2 bore fourteenth children; 1 bore a fifteenth child; 2 bore sixteenth children; 1 bore a nineteenth child.

\* *Edinburgh Medical Journal*, June 1863, p. 1142. "That in the mother (he remarks) which leads to the miscarriage may lead also to the idiocy, and the only connection may be one through a common cause." Again he says, "It frequently happens that between the birth of the idiot and that of the child which precedes or follows, an interval occurs which is much longer than usual, or that after the birth of the idiot permanent sterility appears. . . . Again, when the idiot is born eighteen or twenty-four months after the preceding child, but when for six or seven years thereafter no impregnation occurs, he thought there was reason to suspect that the imperfection in reproductive power, which showed itself in the idiot, had merely another and fuller expression in the subsequent barrenness. And so also when permanent sterility follows. In many cases indications of barrenness preceded the birth of the idiot, and became permanent thereafter."



In short, the great majority of women child-bearing late in life are mothers of considerable families, not women for whom a postponement of the generative orgasm has to be imagined, a circumstance which destroys all shadow of ground for Mason Good's supposition.\*

\* For other corroborative evidence, see Robertson, *Physiology and Diseases of Women*, p. 183. "An examination (says he) of the table naturally suggests to the mind two questions: First, Have women, bearing children above the age of forty-five, generally been married late in life? . . . To the first question I can give only an imperfect, but perhaps a sufficient answer. Of eleven women, three of whom had a child each in her forty-ninth year, and the other eight had each a child above that age, I ascertained that the aggregate number of their children was 114—*i.e.* ten and a fraction for each woman; a fact indicating that they must have married rather early in life. Concerning the age of marriage in two out of the eleven, I possess some little information; the one married at eighteen, had two children before she was twenty-one, and brought forth her fourteenth child in her fiftieth year: the other was married from a boarding-school at a very early age; in her fifty-third year she was delivered of her twelfth child."



## CHAPTER XIV.

CONTRIBUTIONS TO THE ADULT POPULATION BY  
MARRIAGES AT DIFFERENT AGES.

THIS is the great question which political economists have aimed at discussing, however confused and irregular may have been their modes of proceeding. The attentive reader will have already seen how many subsidiary questions intervene between the mere calculation of the number of births by women of different ages and the question of fertility of marriages at different ages, in children that will survive to adult age. This last is the point which political philosophers chiefly wish to solve ; yet several other calculations, to which we have made reference, have been taken and held as if they offered a solution of this great question of population.

Indeed, even now, I can offer nothing positive towards the solution of this important point. It is very desirable it should be settled by the accumulation and analysis of data ; and considering the copiousness of the relative facts, I venture to express a hope that some of our statisticians, especially



those having use of public money and charge of the public records, will undertake the easy task.

The best solution I can offer appears to me to be very trustworthy, and I feel some confidence that actual data will be found to confirm it. It is arrived at by the following process of reasoning. The ascertained fertility of fertile marriages above twenty-five years of age (Tables XL. and XLI.) is so much less than that of those below that no further consideration of the former requires to be made. Besides, the ascertained sterility of marriages above twenty-five is so much greater (Tables XIV. and LXVIII.) than that of marriages under twenty-five, as still further to put out of the competition all marriages above twenty-five.

The quinquenniads which may be regarded as mutual rivals in fertility are the two first—that is, from 15 to 19 inclusive, and from 20 to 24 inclusive. To aid us in deciding between the former and latter we have to inquire into :—1. Their fecundity (or sterility) ; 2. Their fertility ; 3. Their survival of child bearing ; 4. The survival of their offspring ; 5. The healthiness of their offspring.

In all except the second of these particulars, the first quinquenniad is surpassed by the second. Wives married at from 15 to 19 have seven per cent of sterility among them, while wives married at from 20 to 24 appear to have none (Table LXVIII.) This circumstance will make greatly against the fertility of a mass of wives from 15 to 19. Indeed, considering the small excess of the fertility of a mass of wives from



15 to 19 over those of the next quinquenniad (Table VII.), this drawback evidently has great effect. To this drawback has to be added their less chance of surviving a first confinement (Part VII.) Still further, the diminished amount of survival of their offspring (Table LXVI.) has to be taken into account; and after all these, some weight against the early quinquenniad is to be ascribed to the general belief of the greater unhealthiness of their progeny as affecting their survival to adult age, or periods beyond those concerning which we have the numerical statements already referred to.

Although, then, wives of the first quinquenniad have, in virtue of their great perseverance in fertility, a greater total fertility than wives of any other age, I do yet regard the wives of the second quinquenniad—that is from 20 to 24—as most prolific in desirable offspring, as contributing most to the adult population; and this favourable view of the latter arises from the evils just enumerated as attending what we may now justly call premature marriages or marriages of immature women.

As already shown, Sadler went far wrong in favouring the marriages of the elderly. I shall not here discuss his view again, contenting myself with merely quoting his words. “Thus, then, does it plainly appear,” says he, “that among the wealthy as well as the poor the same law of nature prevails; and, consequently, it is universal. As far as the preceding table goes, not only are the marriages more prolific the



longer they are deferred, but the deaths in their offspring are proportionally less numerous; causing, therefore, by the inverse rates of fecundity and mortality, the latter marriages to be far more conducive to permanent increase than the former ones.”\*

On this subject Major Graham has not, so far as I know, entered at any length. But I quote some remarks by him upon it, merely premising that while they are on the whole important and just, they yet appear to proceed on at least one insecure assumption—namely, that the number of births to each generation would necessarily grow less as the age of women at marriage increased—an assumption which is rendered doubtful by the demonstration of the variations, according to age at marriage, of fecundity, fertility, and other important circumstances already mentioned in this chapter. “The proportion of children to a marriage,” says Graham, “and consequently the population, are regulated, not so much or so immediately by the numbers of the people who marry as by the age at which marriage is contracted. The mothers and fathers of nearly half of the children now born are under 30 years of age; and if all the women who attain the age of 30 should marry, and none should marry before that age is attained, the births would decline to about two-thirds and if the marriage age were postponed to 35 the births would fall to one-third part of the present number: so the population would rapidly decline;

\* *Law of Population*, vol. ii. p. 281.



firstly, because the number of births to each generation would grow less ; and secondly, because, as the interval between the births of successive generations would increase, and the duration of life by hypothesis remain the same, the numbers living contemporaneously—in other words, the population—would be farther diminished.”\*

The most valuable contribution to this discussion of which I know, is afforded by the Committee of the Statistical Society of London, to whose labours I have repeatedly made reference. My own remarks at the beginning of this chapter are confirmatory of those of the committee, from whose report I now quote : “ From this abstract (Table LXVI.)† it is obvious, that of the three first periods, the children born of marriages formed in the quinquennial term of life 21-25, are subject to a less rate of mortality than those of the period immediately preceding or immediately following. The rate of mortality in the most advanced period, 31-35, is very irregular, and no doubt arises from the small number

\* Census 1851. *Report*, vol. i. p. xlvii.

† TABLE LXVI.

Years elapsed since Birth of First Child.	Mortality per cent of the Children born to Marriages formed at ages—			
	16-20	21-25	26-30	31-35
10	36·87	37·09	37·89	35·48
20	47·44	43·10	44·36	16·67
30	53·03	43·89	48·53	64·29
40	63·12	57·14	68·00	50·00



of families included in that group. The two preceding series of facts furnish materials for the solution of a very interesting and highly important question—namely, what is the effect of the marriages formed at those different terms of life on the ultimate increase of population? By the first (Table XLI.) of the two preceding abstracts it was found, that the earlier the period of life at which marriage was contracted the greater the number of children born; but by the second abstract (Table LXVI.) a difference is observable in the rate of mortality of the various periods, and this must disturb the results in the first class of facts.

“Let  $a$  represent the results given in the first abstract,  $b$  represent those given in the second; then  $a - \frac{a \times b}{100}$  = the actual increase resulting from each marriage to the population. The following is an abstract of the results thus arrived at:—

TABLE LXVII.

Years elapsed since birth of first child.	Children alive by each Marriage contracted at the following ages.			
	16–20	21–25	26–30	31–35
10	3·19	2·84	2·75	2·22
20	4·04	4·09	3·58	2·50
30	3·95	4·43	3·50	2·50
40	4·00	3·53	1·60	2·00

It hence follows that marriages formed under 25 years of age increase the population more than those formed above that age; and on a close examination it will be found that there is very little difference in this respect



between marriages contracted at ages 16-20 and 21-25, the rate of increase, however, being somewhat higher in the former period: With regard to the last two quinquennial terms at which marriage is formed, it will be seen that the rate of increase is not so great for ages 26-30 as in that immediately preceding, and in the period 31-35 the rate of increase is still less ; in fact, the earlier the period of marriage the greater the increase resulting to the population, the difference between the first and second periods being very little, between the second and third very considerable, about 23 per cent, and between the third and fourth about 20 per cent.

“In the consideration of these facts and observations, although they relate to 1506 families, from which have resulted 8034 births, and of which 4616 children, or 57·46 per cent, are still alive, it must be borne in mind that they include only one class of the community, and may be subject to disturbing influences, such as to destroy their character as a type of the general population ; however, there is reason to suppose that these results may be a more faithful representative of the condition of the whole population, than if they were derived from a like number of facts from either the middling or higher classes of society. On reflection, it will also be found, that the unfruitful marriages are not included in any of these 1506 families, all included being more or less productive. Likewise, the marriages are all those in which one or both the parents are still alive, and consequently the results of fruitful marriages, in which the



parents have died before the lapse of the given period of years brought under review, are excluded. An influence, independent of the relative number of marriages at each age, will further affect the results arising from the varying rates of mortality at the different terms of life, even when equal numbers only at those periods are considered ; and it will follow, that fewer marriages of limited fruitfulness will be excluded from the groups at the younger ages, the effect of which must be to show in the preceding figures a reduced ratio of children at each marriage formed at those periods of life, compared with that which would appear were all cases included. The relative bearing of all the results is therefore so far modified. Also, the children still alive, composing 57·46 per cent of all born, may, subsequent to the period now under observation, and when classified according to the ages at marriage of their parents, show a very different rate of mortality from that indicated in the respective classes by those who have hitherto died, and still more extended observations would be required to show, whether any and what difference exists in the fruitfulness of the marriages in the succeeding generation. Lastly, all these remarks have had reference to the age of the mother only, at birth of her first child.”\*

\* *Journal of the Statistical Society of London*, vol. xi. 1848, p. 224..



## CHAPTER XV.

THE COMPARISON OF THE FECUNDITY AND FERTILITY  
OF DIFFERENT PEOPLES.

I KNOW of no comparisons of the fecundity of different peoples—*i.e.* of the proportional number of married women who bear any children, who are not sterile. To give to such comparison any physiological value, it will be necessary to establish uniformity in the condition of age of the women at marriage, as it has been demonstrated that fecundity varies greatly according to the variations of this circumstance.

Some approach may, however, be made to a comparison, not of the fecundity simply, but of the actual fecundity of the whole marriages in England and Scotland. On this point, the reports of Major Graham and Dr. Stark may be quoted. "A great number of married people," says the former, "have no children living; and it was shown in the previous report, from a limited but perhaps a sufficient number of facts, that about 28 in 100 married pairs had no children residing with them on the census night. From other observations, it may be estimated, however, that not more than 20 in 100 families are childless, and consequently



that about 80 in 100 have children living.”\* This rough estimate of the fecundity of English women tallies very closely with that made for Scotland by Dr. Stark. “Taking,” says he, “two of the largest registration districts of Glasgow, it was found that of 14,523 married persons living together, 11,718 had children living with them; while 2805 had no children with them. This would yield the proportion of 80·686 per cent with children, and 19·314 per cent without children; or, without the decimals, that in every 100 married couples, 81 had children, while 19 had none.”†

I have already, in speaking of twins, shown how fallacious a test of the fertility of a people their frequency probably is. It has been used, however, as an index of such fertility.

Dr. Stark has tried another plan of ascertaining the comparative fertility of England and Scotland. After pointing out that more children are born to each marriage in Scotland than in England, he proceeds as follows:—“But the comparison may be carried further and closer, by ascertaining the exact number of the married women at the child-bearing ages, and comparing their number with that of the legitimate births. This ascertains to a nicety the fact we are in search of—viz. the comparative fruitfulness of the married women in England and in Scotland. In Scotland, in 1861, there were 305,524 married women between the ages of 15 and 45 years; and as, during that year, there occurred

\* Census of 1851, vol. i. p. xliii.

† Census of Scotland, 1861, vol. ii. p. xxxvi.



97,180 legitimate births, it is clear that every 314 married women at the child-bearing ages gave birth to 100 children during the year. In England, on the other hand, during the same year, there were 2,319,641 married women between the ages of 15 and 45 years; and as, during that year, there were born 652,249 legitimate children, it is apparent that every 355 married women at the child-bearing ages gave birth to 100 children. In other words, while Scotland required only 314 married women annually to produce 100 legitimate children, England required 355 married women (or 41 married women more) to produce the same number. These striking facts, therefore, establish the pre-eminent vitality of the Scottish population, and seem also to indicate that nature, in order to compensate for the smaller proportion of marriages, renders the married females more prolific.”\*

It appears to me that here Dr. Stark is satisfied with insufficient evidence. Before settling anything, it is necessary to inquire if the interesting figures quoted can be accounted for in any other way than by supposing a pre-eminent vitality or fertility of the Scottish people; and there appear to me to be several such ways. I agree with Major Graham, who suggests one out of several explanations of this difference between England and Scotland, in considering it to be not necessary to assume that there is any essential difference in the organisation, the fecundity, or the

\* Census 1861. *Report*, vol. ii. p. xxviii.



virtue of the women living north and south of the Tweed.

“The number (says Graham) of children to a marriage appears to be greater in Scotland than in England, and this is held to be a proof that married women are more prolific in Scotland than in England.

“Proceeding upon another basis, the annual number of legitimate children registered in England was 626,506 in the five years 1856-60 ; when the average number of wives of the age 15-55, determined directly from the census returns of 1851 and 1861, was 2,843,374 ; consequently 100 wives bore 22·0 children annually. In like manner it is found that 100 unmarried women bore on an average 1·7 illegitimate children ; that is, 17 children to 1000 women. 100 women, including the married and unmarried, bear 12·3 children annually on an average.

“In Scotland, during the same years, the following proportions were found to exist : 100 wives bore 24·8 children annually, 100 spinsters or widows bore 1·9 illegitimate children ; and 100 women bore 12·0 children, legitimate or illegitimate.

“The wives of Scotland, as well as the spinsters, are apparently more prolific than the corresponding classes in England ; and yet, taken collectively, the women of England are more prolific than the women of Scotland. 1000 English women (age 15-55) bear 123 registered children annually ; while 1000 Scotch women bear 120 children. The difference is slight, but it is in favour of the English women.



“This appears at first sight to be contradictory and paradoxical. It is explained by the circumstance that the proportion of recognised wives in the population is much lower in proportion in Scotland than it is in England, and as the fecundity of wives is to that of spinsters as 13 to 1, a slight difference in the proportions alters the birth-rates of the two populations. The difference in this respect between England and Scotland is great; in England 52 in 100 women of the age 15-55 are wives, 48 only are spinsters and widows; in Scotland the proportions are reversedly 44 recognised wives to 56 spinsters and widows.

“By altering the proportions in Scotland, for instance by transferring 57,608 women from the ranks of the unmarried to the married women, and by transferring 2130 children from the ranks of the illegitimate to the legitimate children, the fecundity of women—of the wives and of the spinsters—of Scotland becomes the same as the fecundity of the corresponding classes in England—namely, wives having children 22·034 per cent, spinsters and widows 1·676; instead of 24·790 and 1·916 per cent; and when the transfer is made, the proportions remaining still show a less excess of women living in the state of marriage in Scotland than in England.”\*

These passages illustrate forcibly the difficulty of establishing a comparison between the fertilities of two countries, even when they are in so many respects

\* *Twenty-seventh Annual Report of the Registrar-General, 1866, p. xxi.*



alike as is England to Scotland. There are two ways of making this comparison justly. I, of course, imply not a mere juxtaposition of the actual fertilities; that is already made; but an estimation of the comparative fertilities; and this word, when applied to a people, cannot be construed so as to exclude the element of fecundity (or sterility).

The first is the direct method. A number of women, married at the same age and under other respectively similar circumstances, are compared with one another, as to their families, after having lived in wedlock, all, the same number of years.

The second or indirect method is difficult and complicated. Of it, Dr. Stark may be said to have made the first step. The further progress to a true result by this method consists in correcting Dr. Stark's result by eliminating from it all errors whose source is suspected; in other words, by correcting it for all known causes of possible error. Of these the chief probably are the fertility of different ages at marriage, and the fecundity of different ages; the mortality of primiparæ and multiparæ at different ages; and the survival of labour at different ages.

For further elaboration of this topic I refer to the sixth part of this volume by Professor Tait.

This completes my remarks on the fertility of married women. But the subject is susceptible of further interesting developments, by an inverted method of proceeding, which I proceed to carry out.



It is evident that the conclusions arrived at in this part, or others still more definite, can alone form a sure basis for speculation in the great questions in political economy regarding population, and the various means of increasing it, or of retarding its excessive growth. And it is to be hoped that the promoters of that science will avail themselves of information which Malthus, Sadler, and their followers, evidently desired ardently to possess.

But it is not to the political economist alone that such information is valuable. It will form an element in the guidance of social life, and will certainly greatly contribute to the wisdom in council of the well-informed medical practitioner.



## PART V.

### ON SOME LAWS OF THE STERILITY OF WOMEN.

BEFORE commencing a discussion on the subject, it is necessary to make some definitions, with a view to avoiding the confusion which extensively prevails, from the neglect of the all-important definition of terms. I might be even more exact than I shall be, and excuse myself from adopting such a seeming improvement, on the ground that further refinement of definition would itself cause confusion in the present stage of advancement of our knowledge.

*Absolute sterility* I shall hold to mean the condition of a woman who, under ordinary favourable circumstances for breeding, produces no living or dead child, nor any kind of abortion.

*Sterility* I shall hold to mean the condition of a woman who, under ordinary favourable circumstances for breeding, adds not even one to the population, or produces no living and viable child.

*Relative sterility* I shall hold to mean the condition of a woman who, while she may or may not be sterile, is, under ordinary favourable circumstances for breeding, sterile in relation to the circumstance of time ; or, in other words, in relation to her age, and the duration of her married life.



## CHAPTER I.

## STERILITY OF MARRIAGES IN THE POPULATION.

UNDER this head, the age at marriage, and the duration of it, are not regarded. We simply compare the number of people living in the married state, without and with living children. The only information I have on this point is derived from the writings of Major Graham,\* and Dr. Stark.† “It is a pity,” says the latter, “that when the census was taken up, a query had not been put to every married woman whether she had borne children. We have at present no means of ascertaining what proportion of the marriages proves unfruitful; and it is no criterion to ascertain the number of married persons who had children living with them on the night of the census. Married persons who had a numerous family may have none with them, because they are grown up, or are absent at schools or trades. We know, however, from other sources, that a considerable proportion of

\* Census of England, 1851, vol. i. p. xliii., of Reports by Messrs. Graham, Farr, and Mann.

† Census of Scotland, 1861. Population Tables and Report, vol. ii. p. xxxvi.



marriages proves unfruitful; and as it was shown that the married women of Scotland produce more children in proportion to their number than the married women of England, it would have been extremely interesting to have ascertained whether that depended on more of the Scottish married women being fruitful."

"As it may," continues Dr. Stark, "however, give a distant approximation, it may be stated, that taking two of the largest registration districts of Glasgow, it was found that of 14,523 married persons living together, 11,718 had children living with them; while 2805 had no children with them. This would yield the proportion of 80·686 per cent with children, and 19·314 per cent without children; or, without the decimals, that in every 100 married couples, 81 had children, while 19 had none. These numbers may be safely taken as the proportion in the town populations, seeing that for each district the proportions came out within a very small decimal fraction of one another; also from the circumstance, that in other tables which have been published in the Registrar-General's Second Detailed Annual Report, relative to the proportions of children borne by mothers at different ages in Edinburgh and in Glasgow, the results of the one town almost exactly corresponded with those of the other."

I now quote from the report of the English Registrar-General:—"A great number of married people have no children living; and it was shown in the previous report, from a limited but perhaps a sufficient number of facts, that about 28 in 100 married pairs



had no children residing with them on the census night. From other observations, it may be estimated, however, that not more than 20 in 100 families are childless, and consequently that about 80 in 100 have children living."



## CHAPTER II.

## STERILITY OF WIVES.

THE wives who do not increase the population may be called sterile. But a wife who has one or several abortions, or who bears one or several dead children, or to whom both of these events happen, adds not a unit to the population ; and such a wife cannot be said to be absolutely sterile. In order to discover the amount of sterility of married women, I proceed on the following plan. I take the registers of Edinburgh and Glasgow for 1855, and find what is the number of first children produced in that year. With this I compare the number of marriages in that year. It is evident that the number of first children only should be counted, for they indicate all the wives who are not sterile. If one living child is born to a marriage, that marriage is not sterile. Further, it is evident that, although the first births in 1855 will not all pertain to the women married in that year, it may be assumed that, if the marriages be nearly the same in number for a few contiguous years, the first births in one year will give the fertility very accurately of any



of the contiguous years. From this fertility the sterility can be easily computed.

Now, in 1855 there were, in Edinburgh and Glasgow, 4447 marriages, and 3722 first deliveries of living children, leaving 725 marriages sterile, or 1 in 6.1. But in these figures are included 75 marriages which did not take place till after the women had passed forty-four years of age, and these will damage the physiological value of the statement, as these 75 women could not be expected to be prolific.

Of women between the ages of fifteen and forty-four inclusive, there were married 4372; among wives of the same ages, 3710 had first children, leaving 654 marriages sterile, or 1 in 6.6. In other words, 15 per cent of all the marriages between fifteen and forty-four years of age, as they occur in our population, are sterile.

The statement of the amount of sterility just given appears to me, from the largeness of the figures used, to be far more valuable than any other I know of. But on account of their great interest, I shall quote the statements of two authors: \*—"In the *Dictionnaire des Sciences Médicales* (vol. vi. p. 245; see also *Neue Abhandlungen der Schwedischen Akademie der Wissenschaften*, vol. xi. p. 70), it is stated," says Sir James

\* Lever's statement I here submit, but I cannot ascribe much value to it, because no evidence is adduced, and because there is an evident numerical error in some part of the passage. He says, "It is found that  $\frac{1}{20}$ -th, or 5 per cent, of married women are wholly unprolific."—*Organic Diseases of Uterus*, p. 5.



Y. Simpson,\* “that Hedin, a Swedish minister, had noticed that in his parish, composed of 800 souls, one barren woman is not met with for ten fertile. It is further stated that Frank asserted, but from what data is not mentioned, that it would be found on investigation, that in most communities containing 300 to 400 couples, at least six or seven would be sterile, without anything in their physical condition to explain the fact. It seems to have been from this assertion of Frank’s, that Burdach, who is almost the only author who even alludes to the matter, has given the general statement that one marriage only in fifty is unproductive (Dr. Allen Thomson’s excellent essay on Generation, in *Todd’s Cyclopædia*, vol. ii. p. 478, footnote).

“For the purpose of ascertaining the point by numerical data, I had a census taken of two villages of considerable size—viz. Grangemouth in Stirlingshire, and Bathgate in West Lothian—the one consisting principally of a seafaring population, and the other of persons engaged in agriculture and manufacture.

“The following form the results in these two places:—Of 210 marriages in Grangemouth, 182 had offspring; 27 had none; or about 1 marriage in 10 was without issue. Of the 27 unproductive marriages, all the subjects had lived in wedlock upwards of 5 years, and in all, the female had been married that period before she reached the age of 45. Again, of 402

\* *Obstetric Works*, vol. i. p. 323.



marriages in Bathgate, 365 had offspring; 37 had none; or about 1 marriage in 11 was unproductive. There were at the same time living in the village 122 relicts of marriages, and of these 102 were mothers; 20 were not mothers; or about 1 in 6 had no family. In all, of 467 wives and widows, 410 had offspring; 57 had none; or about 1 marriage in 8 was unproductive. Of these last 57, 6 had not been 5 years married, and there were other 6 above the age of 45 when married. If we subtract these 12, we have, of 455 marriages, 410 productive; 45 unproductive; or 1 in 10 $\frac{1}{5}$ th without issue.

“Returns such as I have just now adduced are exceedingly difficult to obtain, in consequence of no registers being anywhere kept, so far as I know, that could be brought to bear upon the question. If it had been otherwise, I would here, if possible, have gladly appealed to a larger body of statistical facts, in order to arrive at a more certain and determinate average of the proportion of unproductive marriages in the general community. For the purpose, however, of extending this basis of data, I have analysed, with some care and trouble, the history of 503 marriages, detailed by Sharpe, in his work on the *British Peerage* for 1833. Among British peers there were 401 marriages with issue; 102 without issue; or of 503 existing marriages among British peers in 1833, 74 were without issue after a period of 5 years. Of those who had not yet lived in the married state for 5 years, 28 were still without family; and in Burke's *Peerage* for 1842



there still remained among these 28 marriages, 7 without issue, making 81 as the total number of unproductive marriages among the original 503 ; or the proportion of the unproductive to the productive marriages among this number is, as nearly as possible, 1 in  $6\frac{2}{7}$ . In the above calculation I have excluded 8 unproductive marriages, in which the age of the husband at the date of marriage exceeded 56. These 8, however, ought to be deducted from the original sum of total marriages that were included ; or, in other words, the 503 should be reduced to 495, and then the whole result would stand thus :—Among 495 marriages in the British peerage, 81 were unproductive, or 1 in  $6\frac{1}{5}$  were without any family.” The proportion of unproductive marriages in Grangemouth, Bathgate, and the British peerage, all taken together, was found by Simpson to be 1 in  $8\frac{4}{7}$ .

Dr. West\* states that he found the general average of sterile marriages among his patients at St. Bartholomew's Hospital to be 1 sterile marriage in every  $8\cdot5$ .†

\* *Diseases of Women*, 3d edition, p. 3.

† A statement of the sterility of Esquimaux women is given by Robertson, *Essays and Notes on the Physiology and Diseases of Women*, p. 53.



## CHAPTER III.

## ABSOLUTE STERILITY OF WIVES.

IN order to arrive at the absolute fertility, or, conversely, at the absolute sterility, of the wives in Edinburgh and Glasgow, it is necessary to add to the number of wives bearing first living children the number of those who bear only dead children or abortions.

The number of abortions has been variously estimated by Graunt, Short, Whitehead, and others. The number of children born dead has been the subject of much investigation, among others by Jacquemier, Boudin, and Legoyt. But were our information on these points very exact, it would not help us in this inquiry. For our purpose, the desideratum is not the number of abortions in a number of pregnancies, nor the number of children born dead in a number of births, but the proportional number of married women who produce nought else than abortions or dead children, who, while not absolutely sterile, yet add none to the population. Of this class of wives I know of no estimate.\* I believe they are few, and I leave the

\* The following extract from the work of Dr. West on *Diseases of Women* (3d edit. p. 367), may be of some value. It refers to



statement of the sterile as a near approximation to a correct statement of the absolutely sterile.

the histories of a set of poor women labouring under uterine cancer. "There were but two out of the whole 150 women whose pregnancy had issued merely in abortion."



## CHAPTER IV.

## STERILITY ACCORDING TO THE AGES OF WIVES.

To illustrate the variations of sterility according to age, I bring forward the accompanying Table (LXVIII.)

With the numbers of marriages taking place in Edinburgh and Glasgow in 1855, at different ages of the wives, are compared the numbers of first children born in the same year to wives married at the same ages in that year or previously. The number of sterile wives is got by subtracting the latter figures from the former, and the percentage of sterile marriages is given in the penultimate horizontal line.

So far as the numbers are to be relied upon, we have from this table the interesting results, that about 7 per cent of all the marriages between 15 and 19 years of age inclusive, and as they occur in our population, are without offspring; that those married at ages from 20 to 24 inclusive are almost all fertile; and that after that age sterility gradually increases according to the greater age at the time of marriage.



TABLE LXVIII.—SHOWING THE VARIATIONS OF STERILITY ACCORDING TO THE AGES OF THE WIVES.

Ages of Wives at Marriage . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50, &c.	Total.
Number of Wives . . . . .	700	1835	1120	402	205	110	46	29	4447
First Children . . . . .	649	1905	809	251	96	10	2	...	3722
Sterile Wives . . . . .	51	...	311	151	109	100	44	29	725
Percentage Sterile . . . . .	7.3	...	27.7	37.5	53.2	90.9	95.6	100	16.3
Proportion Sterile, 1 in . . .	13.72	...	3.60	2.66	1.88	1.10	1.05	1.00	6.13



## CHAPTER V.

## EXPECTATION OF STERILITY.

THE main element in the expectation of sterility is the age of the woman at marriage. This has just been described. But, besides this, our statistics suggest to us other laws as to the expectation of sterility. Of these the first is :—

*That the question of a woman's being probably sterile is decided in three years of married life.* For while a large number are fertile for a first time in each of the first three years of married life, only 7 per cent of the fertile bear first children after three years of marriage, or about 1 in 13. (Table LXIX.)

This same table affords us a second law of expectation of sterility :—

*That when the expectation of fertility is greatest, the question of probable sterility is soonest decided, and vice versa.* For our tables show that of the wives married from 20 to 24 who are all fertile, only 6·2 per cent begin to breed after three years of marriage ; while at the other ages, with less fecundity, a greater percentage commences after the completion of the third year of marriage.



TABLE LXIX.—SHOWING THE FERTILITY OF MOTHERS, OF DIFFERENT AGES AT MARRIAGE, COMMENCING AFTER THREE YEARS OF MARRIED LIFE.

Mother's Age at Marriage . . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	Total.
Number of Fertile . . . . .	649	1905	809	251	96	10	2	3722
Number commencing Fertility after being 3 years married . . . . .	63	119	62	27	15	1	...	287
Percentage commencing Fertility after being 3 years married . . . . .	9.7	6.2	7.7	10.7	15.6	10.0	...	7.7
Or 1 in . . . . .	10.3	16.0	13.0	9.3	6.4	10.0	...	13.0



## CHAPTER VI.

## RELATIVE STERILITY.

HERE I take into consideration only those who have borne children—only those who are not sterile. Of course all these wives, if they survive in wedlock, will sooner or later become relatively sterile. Now, I have already shown that the prolongation of fertility was greater according as the age at marriage was less. From this conclusion it is easy to derive one in regard to relative sterility, to the effect that— :

*Relative sterility will arrive after a shorter time according as the age at marriage is greater.* The demonstration of these propositions is arrived at by showing the proportional numbers bearing at different years of married life, according to age at marriage. This is an indirect way of proceeding, but it is the only one I can find available, while I have no documents giving the ages of mothers at marriage, and their ages at birth of last children, the mothers continuing to live in wedlock.



TABLE LXX.—SHOWING THE RELATIVE STERILITY OF A MASS OF WIVES MARRIED AT DIFFERENT AGES AT SUCCEEDING EPOCHS IN MARRIED LIFE.

Age of Mother at Marriage . . . . .	15-19	20-24	25-29	30-34	35-39	Total.
Proportion Sterile about the 5th Year of } Married Life is about 1 in . . . . . }	2.78 35.9	2.61 38.3	1.68 59.4	1.51 66.0	1.19 84.1	2.09 47.9
Proportion Sterile about the 10th Year of } Married Life is about 1 in . . . . . }	2.09 47.9	1.71 58.3	1.39 71.8	1.24 80.8	... ...	1.61 62.1
Proportion Sterile about the 15th Year of } Married Life is about 1 in . . . . . }	1.57 63.8	1.32 75.5	1.10 90.9	1.05 95.5	... ...	1.26 79.2
Proportion Sterile about the 20th Year of } Married Life is about 1 in . . . . . }	1.24 80.4	1.13 88.6	1.01 98.7	... ...	... ...	1.11 89.8
Proportion Sterile about the 25th Year of } Married Life is about 1 in . . . . . }	1.02 97.6	1.00 99.65	... ...	... ...	... ...	1.01 99.03



Table LXX. gives the calculated amounts of sterility at different periods of married life in women married at different ages. It is needless to enter on the method of construction of this Table. It is merely the complement of Table LIV., given already, where full details are stated. I shall only add, that this Table is all calculated for 20 months, with a view to giving the nearest accurate estimate, 20 months being what I have called the time-unit of fertility, the shortest time within which all women may be expected to show fertility if they possess it.

In my remarks on the fertility of elderly women (p. 162) I gave reason for believing that there was no prolongation of the reproductive powers beyond ordinary ages in the case of women married late in life. In the same part (p. 140) I showed that the greater a woman's age at marriage, the shorter is her era of child-bearing; or, in other words, the less is her perseverance in fertility. But in neither of the chapters referred to have I pointed out what Professor Tait has shown (p. 216) regarding the advent of relative sterility. His table (No. LXXXIV.) shows that—

*The older a fertile woman is at marriage, the older is she before her fertility is exhausted; that is, before the advent of relative sterility.*

Similar results are deducible from Table LXXI., to which I refer the reader for actual numbers, not of fertility, as in Professor Tait's, but of sterility. This law, then, of the advent of sterility does not modify the other closely-placed laws already alluded to. It does not touch the question of the fertility of elderly



women, nor the varying perseverance in fertility of women married at different ages ; it merely shows that, although it is true that the older a fertile woman, the shorter is her era of fertility, or the sooner does she arrive at sterility ; yet this era of fertility, shortened in proportion to age at marriage, carries the subject of it into greater actual ages of fertility than are reached by the earlier married.\*

\* I here subjoin a Table identical with the preceding (LXX.), except that it is corrected for sterility, as the similar Tables in Part IV. have been amended.

TABLE LXXI.—SHOWING THE RELATIVE STERILITY OF A MASS OF FERTILE WIVES MARRIED AT DIFFERENT AGES, AT SUCCESSIVE PERIODS IN MARRIED LIFE.

Age of Mother at Marriage .	15-19	20-24	25-29	30-34	35-39	Total.
Proportion Sterile about the 5th year of Married life is about 1 in . . . . .	3·27	2·61	2·25	2·16	1·51	2·46
Or a percentage of . . . . .	30·6	38·3	44·5	46·2	66·0	40·6
Proportion Sterile about the 10th year of Married Life is about 1 in . . . . .	2·25	1·71	1·66	1·44	...	1·74
Or a percentage of . . . . .	44·5	58·3	60·3	69·2	...	57·3
Proportion Sterile about the 15th year of Married Life is about 1 in . . . . .	1·66	1·32	1·14	1·07	...	1·31
Or a percentage of . . . . .	60·3	75·5	87·3	92·9	...	76·2
Proportion Sterile about the 20th year of Married Life is about 1 in . . . . .	1·27	1·13	1·02	...	...	1·13
Or a percentage of . . . . .	78·9	88·6	98·3	...	...	88·7
Proportion Sterile about the 25th year of Married Life is about 1 in . . . . .	1·03	1·00	...	...	...	1·01
Or a percentage of . . . . .	97·4	99·62	...	...	...	99·01



## CHAPTER VII.

## EXPECTATION OF RELATIVE STERILITY.

As a sort of appendix, I produce five tables, giving all the details of the expectation of continued fertility, and, conversely, of relative sterility. These tables not only give data for calculating the chances of relative sterility, but also for calculating the probable number of the family produced in women at different ages becoming relatively sterile. To enter further upon these considerations would be merely to give in writing what is more succinctly stated in the tables themselves.

Lastly, I state a law of relative sterility for which I do not here adduce the numerical proofs, these having already been given in the former part. This law is, that—

*A wife who, having had children, has ceased for three years to exhibit fertility, has probably become relatively sterile; that is, will probably bear no more children; and the probability increases as time elapses.* For the probability of sterility only commences after three years of sterile marriage. Further, the data given in Table XXXIV. show that fertile



TABLE LXXII.—FIFTH YEAR OF MARRIED LIFE.

Number of Child . . . . .	1st	2d	3d	4th	5th	6th	7th	8th	9th	Total.
Wives-Mothers, of Ages 20-24 . . .	13	39	160	31	4	...	...	...	...	247
Proportion of above to 644 surviving Wives Married at from 15-19 is 1 in }	49.5	16.5	4.0	20.8	161.0	...	...	...	...	2.6
Wives-Mothers, of Ages 25-29 . . .	10	82	398	106	13	2	...	...	..	611
Proportion of above to 1686 surviving Wives Married at from 20-24 is 1 in }	168.6	20.5	4.2	15.9	13.0	843.0	...	...	...	2.7
Wives-Mothers, of Ages 30-34 . . .	3	31	147	52	8	2	1	...	...	244
Proportion of above to 1008 surviving Wives Married at from 25-29 is 1 in }	336.0	32.5	6.8	19.4	126.0	504.0	1008	...	...	4.1
Wives-Mothers, of Ages 35-39 . . .	3	12	37	14	1	1	2	1	1	72
Proportion of above to 358 surviving Wives Married at from 30-34 is 1 in }	119.3	29.8	9.7	25.6	358.	358.	179.	358.	358.	4.9
Wives-Mothers, of Ages 40-44 . . .	3	2	11	1	...	...	...	...	...	17
Proportion of above to 179 surviving Wives Married at from 35-39 is 1 in }	59.6	89.5	16.3	179.	...	...	...	...	...	10.5
Total Wives-Mothers, of Ages 20-44	32	166	753	204	26	5	3	1	1	1191
Proportion of above to 3875 surviving Wives Married at from 15-39 is 1 in }	121.1	23.3	5.1	19.0	149.	775.	1291.6	3875.	3875.	3.2



women bear a child, on an average, about every two years, so long as they remain fecund. The data given in Table XXXVIII. show that successive children in a family succeed one another with an average interval of about 20 months. To these propositions I have to add the general consent, shown in the same place, that fertile wives breed generally every two years ; consequently, that no class breeds, though individuals do, at shorter intervals ; and no class breeds at longer intervals, though individuals do so. Considering these different statements, it is apparent to the student that there is no room left for any but a very inconsiderable number of women to breed at longer intervals than two years. For were there any considerable number of wives breeding at longer intervals, the averages just given would be far overpassed. And some of these averages are, as already shown, considerably less than were believed to be the true averages by writers who were not thinking of the law now demonstrated, but of the ordinary rate of time-fertility of married women.

Besides being of evident intrinsic value, the conclusions here arrived at will afford to medical men means of estimating the utility of the many vaunted methods of curing sterility which are now much in vogue, and which, considering the nature of the condition to be cured, justly excite anxiety for the honour of the profession in the minds of its best friends.



TABLE LXXIII.—TENTH YEAR OF MARRIED LIFE.

Number of Child	1st	2d	3d	4th	5th	6th	7th	8th	9th	10th	11th	Total.
Wives-Mothers, of ages 25-29 Proportion of above to 594 sur- viving Wives Married at from 15-19, is 1 in . . . .	...	1 594.	14 42.4	30 19.8	78 7.6	51 11.6	8 74.2	4 148.5	...	...	...	186 3.2
Wives-Mothers, of ages 30-34 Proportion of above to 1528 sur- viving Wives Married at from 20-24, is 1 in . . . .	1 1528.	4 382.	17 89.9	55 27.8	148 10.3	105 14.5	34 44.9	11 138.9	3 509.3	1 1528.	2 764.	381 4.0
Wives-Mothers, of ages 35-39 Proportion of above to 902 sur- viving Wives Married at from 25-29, is 1 in . . . .	...	2 451.	4 225.5	19 47.5	60 15.	48 18.8	13 69.4	5 180.4	2 451.	...	...	153 5.9
Wives-Mothers, of ages 40-44 Proportion of above to 313 sur- viving Wives Married at from 30-34, is 1 in . . . .	...	...	5 62.6	11 28.5	10 31.3	6 52.2	2 156.5	1 313.	...	1 313.	...	36 8.7
Total Wives-Mothers, of ages 25-44 Proportion of above to 3337 sur- viving Wives Married at from 15-34, is 1 in . . . .	1 3337.	7 476.7	40 83.4	115 29.0	296 11.3	210 15.9	57 58.5	21 158.9	5 667.4	2 1668.5	2 1668.5	756 4.4



TABLE LXXIV.—FIFTEENTH YEAR OF MARRIED LIFE.

Number of Child	1st	2d	3d	4th	5th	6th	7th	8th	9th	10th	11th	12th	13th	Total.
Wives-Mothers, of ages 30-34 Proportion of above to 532 surviving Wives Married at from 15-19, is 1 in .	1 532	3 177.3	2 266	6 88.7	11 48.4	18 29.6	24 22.2	28 19.0	18 29.6	2 266	1 532	1 532	1 532	116 4.6
Wives-Mothers, of ages 35-39 Proportion of above to 1360 surviving Wives Married at from 20-24, is 1 in .	... ...	... ...	5 272	4 340	18 75.5	32 42.5	53 25.6	41 33.2	29 46.9	14 97.1	2 680	1 1360	1 1360	200 6.8
Wives-Mothers, of ages 40-44 Proportion of above to 782 surviving Wives Married at from 25-29, is 1 in .	... ...	... ...	1 782	2 391	4 195.5	7 111.7	12 65.2	14 55.9	2 391	1 782	... ...	... ...	... ...	43 18.2
Wives-Mothers, of ages 45-49 Proportion of above to 262 surviving Wives Married at from 30-34, is 1 in .	... ...	... ...	... ...	1 262	1 262	1 262	... ...	1 262	2 131	... ...	1 262	... ...	... ...	7 37.4
Total Wives-Mothers, of ages 30-49 . . . Proportion of above to 2936 surviving Wives Married at from 15-34, is 1 in .	1 2936	3 978.6	8 367	13 225.8	34 86.3	58 50.6	89 33.0	84 34.9	51 57.5	17 172.7	4 73.4	2 1468	2 1468	366 8.0



TABLE LXXV.—TWENTIETH YEAR OF MARRIED LIFE.

Number of Child . . . .	4th	5th	6th	7th	8th	9th	10th	11th	12th	13th	Total.
Wives-Mothers, of ages 35-39 .	...	...	2	5	5	17	15	9	3	...	56
Proportion of above to 477 sur- viving Wives Married at from { 15-19, is 1 in . . . .	...	...	238.5	95.4	95.4	28.0	31.8	53.	159.	...	8.5
Wives-Mothers, of ages 40-44 .	1	1	2	9	14	28	8	13	2	2	80
Proportion of above to 1171 sur- viving Wives Married at from { 20-24, is 1 in . . . .	1171.	1171.	585.5	130.	83.6	41.8	146.4	90.1	585.5	585.5	14.6
Wives-Mothers, of ages 45-49 .	...	...	...	...	...	3	...	1	1	...	5
Proportion of above to 649 sur- viving Wives Married at from { 25-29, is 1 in . . . .	...	...	...	...	...	216.3	...	649	649	...	129.8
Total Wives-Mothers, of ages 35-49	1	1	4	14	19	48	23	23	6	2	141
Proportion of above to 2297 sur- viving Wives Married at from { 15-29, is 1 in . . . .	2297.	2297.	574.2	164.	121.	47.8	99.9	99.9	383.	1148.	16.3



TABLE LXXVI.—TWENTY-FIFTH YEAR OF MARRIED LIFE.

No. of Child	10th	11th	12th	13th	14th	15th	16th	17th	Total.
Wives-Mothers of Ages 40-44 . Proportion of above to 408 sur- viving Wives Married at from 15-19 is 1 in . . . . .	2 204·	2 204·	1 408·	1 408·	...	...	...	...	6 68·
Wives-Mothers of Ages 45-49 . Proportion of above to 961 sur- viving Wives Married at from 20-24 is 1 in . . . . .	... ...	1 961·	... ...	.. ...	... ...	... ...	... ...	1 961·	2 480·5
Total Wives-Mothers of Ages 40-49 Proportion of above to 1369 sur- viving Wives Married at from 15-24 is 1 in . . . . .	2 684·	3 456·	1 1369·	1 1369·	...	...	...	1 1369·	8 171·



## PART VI.

### NOTE ON FORMULÆ REPRESENTING THE FECUNDITY AND FERTILITY OF WOMEN.\*

By Professor TAIT.

1. DR. MATTHEWS DUNCAN having requested me to point out to him some simple method of comparing the fertility of different races, I endeavoured, as a preliminary step, to represent by formulæ some of the chief results which he has obtained in his very lucid and elaborate papers recently read to this Society and printed in their Transactions for 1863-4 and for the present session. Some of the formulæ which I have obtained are so simple, and accord so well with the tables, that I have thought them worth bringing before the Society. Of course it must be understood that I advocate no theory, and pretend to no physiological knowledge of the question. I merely try to represent, in a simple analytical form, the contents of some of Dr. Duncan's tables.

\* These chapters are here re-published from this year's volume of the *Transactions of the Royal Society of Edinburgh*, by the permission of the Council of the Society, and with the kind acquiescence of Professor Tait. Their interest and importance are such as to make me very glad to be able to present them, in this place, to my readers.—J. M. D.



2. To prevent misconception, let us begin by defining the terms *fecundity* and *fertility* as they will be used in this note, unless qualified in some manner.

By *fecundity* at a given age we mean the probability that during the lapse of one year of married life, at that age, pregnancy, producing a living child, will ensue. This is, in all likelihood, modified in each individual woman by the previous duration of marriage (see § 10 below). But at present, in dealing with the mass of wives, we omit this consideration. We do not require, in our calculations, to consider any questions connected with the duration of life of husband and wife, of the length of time the child may live, etc., as the numbers in the tables are already influenced by such causes. The numbers in the tables do not usually denote the fecundity as above defined, but are quantities proportional to its values.

By *fertility*, at any age, we mean the number of children which a married woman of that age is likely to have during the rest of her life, or some numerical multiple of it.

The subject divides itself into three heads—(I.) The fertility and fecundity of the mass of wives; (II.) Their value for the average individual; (III.) The *relative* fertility and fecundity of different races.

These we proceed to consider in order.



## CHAPTER I.

## FERTILITY AND FECUNDITY OF THE MASS OF WIVES.

3. If  $f_t$  represent the fecundity, and  $F_t$  the fertility at the age of  $t$  years, the ordinary laws of probability, if applicable to this question, give us the expression—

$$F_t = f_t + f_{t+1} + \dots + f_{49} = \sum_t^{50} f_t$$

assuming that sterility arrives about the age of fifty.

Before going further, it may be well to verify this formula by comparison with the tables, so that we may be assured of the validity of our reasoning.

Dr. Duncan gives (*Trans. R.S.E.*, 1863-4, p. 358)\* the following numbers for the wives in Edinburgh and Glasgow, *taken as a whole*:—

TABLE LXXVII.—SHOWING FECUNDITY OF WIVES TAKEN AS A WHOLE AT DIFFERENT AGES.

Age . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Fecundity	50	41·8	34·6	26·6	20·4	8	1·3

The two last numbers are probably not so accurate

\* Also p. 19 of this volume.



as the others—one from vague statements as to “forty years of age ;” the other on account of some omissions noticed in a footnote to the table. As, unfortunately, we cannot get data for each year separately, we can only test the above formula for intervals of five years. The numbers just given may therefore be taken as *proportional* to  $f_{17}$ ,  $f_{22}$ ,  $f_{27}$ ,  $f_{32}$ ,  $f_{37}$ ,  $f_{42}$ , and  $f_{47}$  respectively.

4. We may now construct the second line of the following table, according to the formula above, by adding to the number for any quinquennial period all those which follow it :—

TABLE LXXVIII.—SHOWING FERTILITY CALCULATED FROM TABLE LXXVII.

Age . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Fertility	182·7	132·7	90·9	56·3	29·7	9·3	1·3
$F_t$	12	8·9	6	3·7	2	0·6	0·1

The numbers in the last line are proportional to those in the second, on the assumption that a woman of 15-19 will have a family of twelve.

Dr. Duncan quotes (*Trans. R.S.E.*, 1865-6, p. 302)\* from the journal of the Statistical Society the following table of values of  $F_t$  for the mass of married women in the district of St. George's-in-the-East. This is,

\* Also p. 139 of this volume.



unfortunately, not quite comparable with the last, as the quinquenniads differ by one year of age; and, besides, the ages at marriage differ in the different columns. But there seems to be no attainable table so nearly approaching what we require for comparison.

TABLE LXXIX.—SHOWING FERTILITY AT DIFFERENT AGES.

Age	16-20	21-25	26-30	31-35
$F_t$	10·85	8·24	5·00	4·00

Neglecting the difference of the quinquenniads in the two tables, and taking 11 instead of 12, for the sake of direct comparison, as the value of  $F$  at 15-19 in the first, we have—

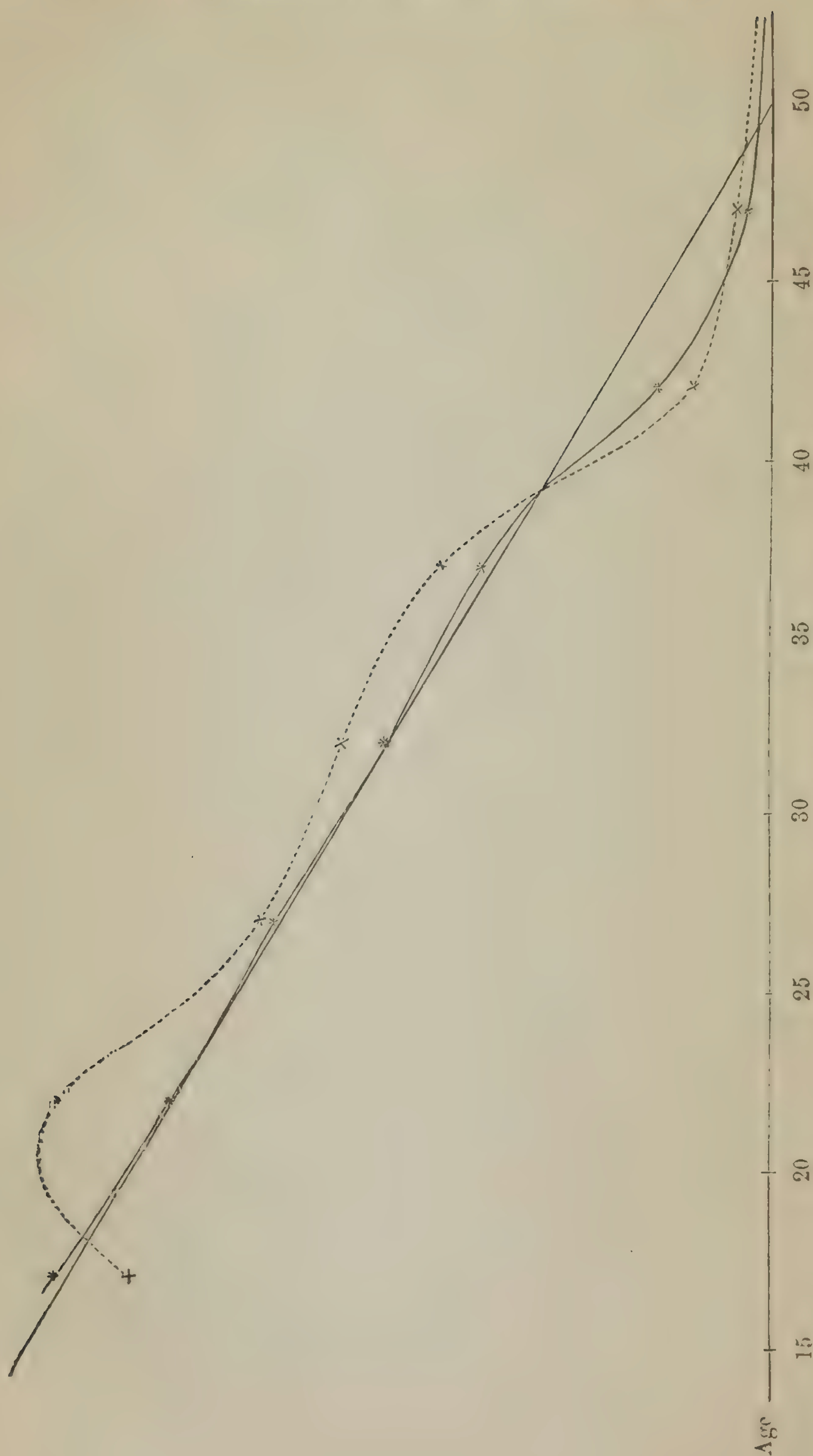
TABLE LXXX.—SHOWING A COMPARISON OF TABLES LXXVIII AND LXXIX.

Calculated	11	8·16	5·5	3·4	1·9	0·55
Observed	10·85	8·24	5·0	4·0		

These numbers agree as well as could possibly be expected.

5. If we project the numbers above given for  $f_{17} \dots f_{47}$ , and try to represent the values of  $f$  for all ages by the ordinates of a curve, whose abscissæ denote the corresponding ages, we have the continuous curve of the following diagram.





## FECUNDITY AT DIFFERENT AGES.

The continuous curve is founded on Edinburgh and Glasgow Tables. The dotted curve on Tables of Sterility.  
 The straight line from the formula  $k(50 - t)$ .



The straight line, which almost coincides with the continuous curve—at least from the age of 17 to that of 40—and whose departure from it above that age must depend to some extent on the defects of the table pointed out in § 3, intersects the axis at 50. We may obviously assume it as very nearly representing the tables. And we can therefore express the value of  $f_t$  by a number proportional to  $50-t$ . Thus—

$$f_t = k (50 - t)$$

(where  $k$  is a *number*, whose value we can easily find), is a simple formula very closely representing the tabulated results.

6. But  $F_t$  can now be represented in a form almost as simple. For—

$$\begin{aligned} F_t &= f_t + f_{t+1} + \dots + f_{49} \\ &= k \left\{ (50-t) + (49-t) + \dots + 1 \right\} = \frac{1}{2} k (50-t) (51-t) \\ &= \frac{1}{2} k (50-t)^2, \text{ nearly enough for our purpose.} \end{aligned}$$

7. Thus it appears that—

*Fecundity is proportional to the number of years a woman's age is under fifty; and*

*Fertility at that age is proportional to the square of the same number.*

8. To show, numerically, how closely these formulæ represent the tables is of course easy.



TABLE LXXXI.—SHOWING COMPARISON OF OBSERVED  
FECUNDITY WITH STATEMENT IN SECTION 7.

Age . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Dr. Duncan	50·0	41·8	34·6	26·6	20·4	8·0	1·3
$\frac{3}{2}(50-t)$	49·5	42·0	34·5	27·0	19·5	12·0	4·5

TABLE LXXXII.—SHOWING COMPARISON OF OBSERVED FER-  
TILITY WITH STATEMENT IN SECTION 7, AND WITH THE  
PROCESS OF SECTION 3.

Age . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Dr. Duncan	10·85	8·24	5·00	4·00			
Calculated from $f_t$ as in (4)	11·0	8·16	5·5	3·4	1·9	0·55	0·09
$\frac{1}{10}(50-t)^2$	10·98	7·84	5·29	3·24	1·69	0·64	0·09

9. *Example.*—As an application of the formula, let us suppose a woman, who was married ten years ago at the age of twenty, to have now five children :—

At marriage . .  $F_{20} = \frac{1}{2} k (50 - 20)^2 = 450 k$   
At present . .  $F_{30} = \frac{1}{2} k (50 - 30)^2 = 200 k$

But the difference  $F_{20} - F_{30}$ , or  $250 k$ , represents five children. Hence  $F_{30}$ , or  $200 k$ , represents four more. So that her family will probably amount to nine.

10. As illustrating the subject further, I append



portions of another of Dr. Duncan's tables (*Trans. R.S.E.*, 1865-6, p. 306),\* with formulæ for comparison founded on the type  $f_t = k (C - t)$ .

TABLE LXXXIII.—SHOWING THE MODIFICATION PRODUCED BY AGE AT MARRIAGE UPON THE FORMULA REPRESENTING FECUNDITY.

Age at Marriage		Age 20-24	Age 25-29	Age 30-34	Age 35-39	Age 40-44	Age 45-49
15-19	Table . . . . .	64·1	52·1	36·2	19·6	2·4	
	3 (43—age) . . . .	63·0	48·0	33·0	18·0	3·	
20-24	Table . . . . .	...	61·7	41·7	24·5	11·4	0·3
	3 (46—age) . . . .	...	57·0	42·0	27·0	12·0	0·0
25-29	Table . . . . .	...	...	40·6	28·2	9·1	1·3
	2·66 (47·5—age) .	...	...	41·4	28·0	14·7	1·4
30-34	Table . . . . .	...	...	...	34·0	19·2	4·5
	3 (48·5—age) . . .	...	...	...	34·5	19·5	4·5

These formulæ seem to represent the tables pretty closely—with the exception of a solitary number for those married at 25-29 —and, if they may be trusted, indicate a very curious result. Of course, when the fecundity is given by an expression of the form  $k (C - t)$ , C is the age at which sterility arrives.

Now, it appears that we have for wives married at

	Fecundity.
15-19	$k (43\text{—age})$
20-24	$k (46\text{—age})$
25-29	$k (47·5\text{—age})$
30-34	$k (48·5\text{—age})$

\* Also p. 148 of this volume.



In words, *the advent of sterility\* is hastened by early marriage.*

Thus sterility occurs according to the following table :—

TABLE LXXXIV.—SHOWING THE AGE AT MARRIAGE, AND OF THE ADVENT OF STERILITY.

Age at Marriage.	Age of Sterility.
15—19	43
20—24	46
25—29	47·5
30—34	48·5

This is singular enough, and seems to be well borne out by the tables, since the age of sterility is *uniformly* later as the age at marriage is greater. But, of course, far more extensive observations must be made and discussed before such a point as this can be settled.

Accepting it, however, for the present, we may calculate from the last table, and the table of fecundity already given, the whole fertility as depending on the age at marriage. For, if  $t$  be the age at marriage,  $C_t$  the corresponding age of sterility—

$$\text{Whole fertility} = \frac{1}{2} k (C_t - t)^2.$$

In this formula  $k$  is to be found. But we have

$$\text{Fecundity at marriage} = f_t = k (C_t - t).$$

$$\text{Hence, whole fertility} = \frac{1}{2} f_t (C_t - t).$$

If we accept 10 children as representing the whole fertility at 15-19, which seems a reasonable assumption, we have

$$10 = \frac{1}{2} f_{17} (43 - 17) = 13 f_{17}.$$

---

\* This is the relative sterility of other parts of this work.—J. M. D.



Hence  $f_{17} = \frac{10}{13}$ , from which (by proportion) the other values of  $f$  are easily found. Hence—

TABLE LXXXV.—SHOWING THE INFLUENCE OF THE ADVENT OF STERILITY UPON THE WHOLE FERTILITY OF MARRIAGE.

Age at Marriage.	Whole Fertility.	F <sub>t</sub> .
15–19	10·	10·
20–24	7·7	7·4
25–29	5·5	5·0
30–34	3·4	3·1

The last column has been added for comparison, so as to show how the later advent of sterility in the more advanced marriages increases the fertility.

It may be well to notice that the interpretation of the expression  $f_{17} = \frac{10}{13}$  is, that *a wife of 15-19 will, on the average, become pregnant at 1·3 years after marriage*—that is, she will have a child within about two years of marriage. This limit of time depends, however, on our assumption of 10 children as the measure of the fertility at 15-19, and childless marriages are included in the data. Dr. Duncan gives (*Trans. R.S.E.*, 1865-6, p. 297)\* 1·52 as the average interval between marriage and the birth of the first child. The reason of the discrepancy is of course this, that in our calculation the mass of wives is considered, and in Dr. Duncan's only fruitful marriages are taken account of.

\* Also p. 128 of this volume.



## CHAPTER II.

FECUNDITY AND FERTILITY OF THE AVERAGE  
INDIVIDUAL.

11. IF we endeavour to derive formulæ of a similar character from the tables of sterility, the results are not quite so simple. Thus we find (*Trans. R.S.E.*, 1865-6, p. 319)—\*

TABLE LXXXVI.—SHOWING THE PERCENTAGE OF STERILITY  
IN WOMEN MARRIED AT DIFFERENT AGES.

Age at Marriage	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Percentage Sterile	7·3	0	27·7	37·5	53·2	90·9	95·6
Percentage not Sterile	92·7	100	72·3	62·5	46·8	9·1	4·4

From the manner in which this table was formed, it would appear that we are justified in taking the numbers in the last line as proportional to the average fecundity at the respective ages. But the curve representing these numbers differs considerably more from a straight line than that derived from the other tables. It is the dotted curve in the figure.

\* Also p. 193 of this volume.



It is true that if we consider the loose way in which women from 30 to 40 call themselves 30, and those from 40 to 50 call themselves 40, we might expect the smaller ordinates belonging to higher ages to be pushed back as it were towards 30 and 40, thereby apparently accounting for the two depressions which appear in the curve about those ages. That this is no fancied explanation may be gathered from the following scandalous facts recorded in the Census Report of 1851, p. 24 :—

In 1841 the number of girls, of ages 10-15 was 1,003,119

But in 1851 these had become young women

aged 20-25, and numbered . . . . . 1,030,456

This number, when corrected from the tables of mortality, obviously includes about 140,000 women whose ages had increased by less than 10 in ten years.

Again, in 1841 the number of women aged 20-25

was . . . . . 973,696

But in 1851 those who had reached 30-35 were

only . . . . . 768,711

indicating at first sight a fearful death-rate, but really showing how strong is the desire to be considered as remaining under the magic limit of thirty years of age.

To complete the examination, however, let us see how far these data from sterility agree with the formula deduced by the other processes.



TABLE LXXXVII.—SHOWING COMPARISON OF FECUNDITY  
AS DERIVED FROM STERILITY, WITH THE STATEMENT IN  
SECTION 7.

Age . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Percentage not Sterile	92·7	100	72·3	62·5	46·8	9·1	4·4
$3\frac{3}{7}(50-\text{age})$	113·1	96	78·9	61·8	44·5	27·4	10·4

It is easy, of course, to construct a formula to represent any series of numbers, but unless it be simple it is of little use; and the approximation we have got seems close enough, if we remember the almost certain deficiencies in the numbers for the two highest ages, and the immaturity, etc., which may easily be supposed to account for that at 15-19. But there is another cause which may serve to account for part of the discrepancy, as in fact Dr. Duncan's table shows. This is that plural births are not eliminated.

In fact, at age 20-24 there are a good many more children per annum than mothers in his table, which thus virtually assumes that no woman of 20-24 is sterile. This accounts for the great rise in the (dotted) curve at the age of 22.

By the process of (3) we form the first line of the following table. The second is formed on the type  $F_t = \frac{1}{2} k (50 - t)^2$ .



TABLE LXXXVIII.—SHOWING COMPARISON OF RESULTS RESPECTING FERTILITY DERIVED BY APPLYING THE PROCESS OF SECTION 3 TO TABLE LXXXVI., WITH THE STATEMENT IN SECTION 7.

Age . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
$F_t$ from $f_t$ observed	387·8	295·1	195·1	120·8	60·3	13·5	4·4
$\frac{19}{50} (50-t)^2$	414	298	202	123	64	24	4

This coincidence also is close enough, and would be still closer if we had the numbers for  $f_{50}$  and upwards, as the smaller numbers in the table, where the deficiency lies, would thus be increased proportionally much more than the larger ones.



## CHAPTER III.

## RELATIVE FERTILITY OF DIFFERENT RACES.

12. WE may apply the above results to compare the fertility of different races—a problem of considerable interest. We shall not attempt a rigorous solution, for the application of which, indeed, we have no sufficient data; but shall make one or two postulates, which will probably be easily admitted, and which will enable us to avoid complication.

13. Suppose that for ten or fifteen years we may consider the number of marriages at any given age to remain practically unaltered, we may then consider *the births in any one year as represented by the total fertility of those married in that year*. That is, the children born in that year of mothers married at 30-34, for instance, are due to those married last year, the year before last, and so on for fifteen years back, at the age of 30-34; and as the number is supposed nearly constant for some years, we have the fertility of *all* for one year (very nearly) by calculating the total fertility for the rest of their lives of those married in that year. As population, and with it the number of marriages, is generally increasing, this process will slightly exaggerate



the numbers sought; but, in *comparing* two growing countries, such as England and Scotland, no perceptible error will be introduced.

14. We next assume that the *law of fertility as depending on age is the same in the two countries compared*. That is, we assume that

$$\frac{F'_t}{F_t} = \frac{F'_{t+1}}{F_{t+1}} = \frac{F'_{t+2}}{F_{t+2}} = \text{etc.} = e,$$

where  $e$  is some definite number; and  $F_t$ ,  $F'_t$  represent the fertility in the two races at age  $t$ .

This will evidently be the case if the fertility be really expressible, as above, in the form

$$F_t = \frac{1}{2} k (50 - t)^2,$$

for two such expressions can only differ through the number  $k$ ; unless indeed the age at which sterility comes on, here represented by 50, should happen to be greatly different for different races. On this point we have no information.

15. Let, then,  $\mu_t$  be the number of marriages of women at  $t$  years of age in any one year,  $\beta$  the number of legitimate births in a year, we have by the above postulates

$$\beta = \sum \mu F = \mu_{15} F_{15} + \mu_{16} F_{16} + \dots + \mu_{49} F_{49}.$$

For another country

$$\beta' = \sum \mu' F' = e \sum \mu' F,$$

where  $e$  is the ratio of the fertility of the second race to that of the first. These equations give

$$e = \frac{\beta'}{\beta} \cdot \frac{\sum \mu F}{\sum \mu' F}$$



where the *absolute* fertility of either country is no longer involved, so that we may employ for the values of  $F$  the expressions in § 4, or those in § 8.

16. *Example.*—From the Registrar-General's reports for England and Scotland last published, we extract the following data :—

TABLE LXXXIX.—SHOWING NUMBER OF MARRIAGES AND NUMBER OF LEGITIMATE CHILDREN IN ENGLAND AND SCOTLAND IN ONE YEAR.

	England, 1864.	Scotland, 1862.
No. of Marriages . . .	180,387	20,597
Legitimate Births . . .	692,827	96,693

TABLE XC.—SHOWING PERCENTAGE FOR EACH QUINQUENNIAD OF WOMEN MARRIED IN ONE YEAR IN ENGLAND AND SCOTLAND, AND THE CORRESPONDING FERTILITY FROM TABLE LXXXVIII.

	15-20	20-25	25-30	30-35	35-40	40-45	45-50
England . . .	13·60	49·74	19·55	7·28	3·89	2·67	1·57
Scotland . . .	13·07	46·28	24·13	8·54	4·36	2·03	0·95
And we assume, in accordance with §§ 8 and 9—							
$F$ proportional to	12·0	8·9	6·0	3·7	2·0	0·6	0·1

$$\text{This gives } \Sigma \mu F = \frac{20,597}{100} [12 \times 13.07 + 8.9 \times 46.28 + \dots] \\ = 20,597 \times 7.55 \text{ for Scotland.}$$

$$\text{Also } \Sigma \mu' F = 180,387 \times 7.595 \text{ for England.}$$

$$\text{Hence } e = \frac{692,827}{96,693} \cdot \frac{20,597}{180,387} \cdot \frac{7.55}{7.595} = 0.812 \text{ nearly.}$$



A more accurate comparison would be obtained by employing the average number of marriages at various ages for five or ten consecutive years, instead of those in any one year, as above, which are liable to considerable fluctuations. But we have not data enough. It would appear, then, that *the absolute fertility of the mass of married women in England is only about 80 per cent of that in Scotland.*

That the fertility is less in England than in Scotland has been shown by the Registrar-General for Scotland (*Report*, 1866). But he makes the ratio considerably greater than the preceding estimate.

It is to be observed that if the insinuations we sometimes hear about Scottish marriages have any foundation in fact, their consideration would tend to make the difference in fertility between the two countries even greater than that just given ; for legitimation *per subsequens matrimonium* does not put a child's name on the Registrar's books.

17. The fact that in England and Scotland the quantities  $\Sigma \mu F'$  and  $\Sigma \mu F$  are almost exactly proportional to the number of marriages in the two countries, shows that, although Scottish women, as a rule, marry later in life than English women, the long period (25-40) during which their marriage-rate exceeds that in England, as compared with the shorter period (20-25), during which it falls behind, almost makes up for the diminished fertility at the more advanced age.

18. It only remains to construct the values of the



quantities  $F_t$  for each country, taken, of course, from the mass of the wives.

As before (§ 15), we should have had

$$\beta = \Sigma \mu F$$

if we had used proper absolute values of  $F$ . But we used the numbers 12, 8·9, 6, etc., which are obviously too large. Reducing them all in the ratio  $\epsilon$  to 1, and substituting for  $\beta$ , etc., their values, we get

$$96,693 = 20,597 \times 7\cdot55 \epsilon.$$

This gives  $\epsilon = 0\cdot622$  ;

from which we construct the following table :—

TABLE XCI.—SHOWING COMPARATIVE FERTILITY OF A MASS OF WIVES IN ENGLAND AND SCOTLAND, TAKING ACCOUNT OF THE AGE AT MARRIAGE.

	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Scotland . . .	7·44	5·54	3·77	2·30	1·24	0·37	0·06
England . . .	6·04	4·49	3·02	1·87	1·01	0·30	0·05

19. In conclusion, it may merely be repeated that we have attempted no elaborate or exact inquiry into this question ; indeed the utter insufficiency of data would have rendered such a proceeding absurd ; and we have, for the same reason, abstained from employing some of our own results, such as those of § 10, in modifying the earlier ones, by the help of which they have been arrived at. Thus, for instance, we should be led by the results of § 10 to use in the formulæ of §§ 5, 6, a number somewhat less than 50, as



corresponding to the average age of sterility. As in all questions of average, the value of our deductions in this matter is mainly dependent on the extent and accuracy of our data, and it is sad to think that the enormous blue-books which load our shelves contain so much painfully-elaborated information which is of no use, and so little of those precious statistics which would at once be easy of acquirement and invaluable to physiologists.







## PART VII.

### ON THE MORTALITY OF CHILDBED AS AFFECTED BY THE NUMBER OF THE LABOUR AND THE AGE OF THE MOTHER.

THERE are two important questions regarding the mortality of lying-in women which certainly have not received the amount of attention which they deserve. They are interesting and important, not only in themselves, but also, in a high degree, on account of their bearing upon topics which are constantly discussed without taking into account the great light and influence which the answers to them might bring upon such topics.

The questions I allude to are :—Does the number of a woman's pregnancy regulate in any degree the mortality to be expected from lying-in? Does the age of the childbearing woman regulate in any degree the mortality accompanying this function?

Analogous questions in regard to some surgical operations and to some diseases have been discussed, and not without good results. This circumstance renders it the more extraordinary that the questions I have proposed regarding parturition have stimulated so little inquiry. The topics of childbearing and of its mortality, and particularly the indefinite disease puerperal fever, are among the most interesting and



carefully studied in the whole range of medicine, and the neglect of the two questions named I can attribute only to the want of materials for their settlement. Yet I do not hesitate to say, that had the profession set these questions before them in their simplicity and importance, materials would ere now have been found or accumulated, and their most desiderated solution satisfactorily effected ere this time.

I regret that at present I know of no data sufficient satisfactorily to decide the questions raised. Yet I shall lay before the profession such as I have collected. They are deficient in point of number and of precision. Had the numbers been much larger the results would have had value in spite of the want of precision in the data as arranged for comparison. The element of want of precision consists mainly in the comparison of different pregnancies not being confined to women of the same age, or *vice versa*. This condition is of course necessary to ensure against a probable source of error, the amount of which is unknown, consisting in the disturbing influence of age or of the number of the labour, upon mortality.

It is well known that a large amount of puerperal mortality is produced by that indefinite class of diseases unphilosophically and injuriously combined under the name of puerperal fever. So important is this class of diseases that it appears to me worth while to discuss separately the influence of age on the mortality from them. The same should be done for all causes of death in or after labour as soon as data are collected. In the



case of puerperal fever some data are at hand. It is not my object at present to enter on the vexed questions in reference to this erroneously so-called fever. No doubt many grave and cardinal errors prevail regarding this class of diseases, and statistics may contribute aid to demolish some of them. The invasion of this disease is well known to be described by a class of obstetricians as an "accident." To remove it from this category is a just object of ambition. To some extent this has already been effected by Sir J. Y. Simpson, who has shown that it is subject to the law of the duration of labour.\* The object will be further promoted if it can be shown to be under a law of the number of the pregnancy, or of the age of the mother, or of both.

\* *Obstetric Works*, vol. i. p. 530. See also *Edinburgh Medical Journal* for July 1857.



## CHAPTER I.

THE RELATION OF THE NUMBER OF THE LABOUR TO  
THE MORTALITY FROM PUERPERAL FEVER.

It may at first sight appear unnatural to enter upon a special kind of mortality in childbed, before describing the whole mortality in childbed. And it is worth while to consider this point briefly, to show that, at least with the means at my disposal, the influence of age is better and more securely observed in this special kind of mortality than in the whole mortality of childbed. For, in proportion as puerperal fever has the quality of an accident, as many obstetricians believe, so will the unalloyed influence of the number of the pregnancy upon its occurrence be observable. Were it evidently not an accident, but due to this cause or the other cause, it would be more and more difficult to eliminate the influence of such causes upon the mortality with a view to arriving at the results produced by the number of the pregnancy. There are, specially in many first labours, such evident and direct causes of death in many cases that the influence of the number of the pregnancy can make no alteration in the fate of the mother. Such cases, in however great numbers, can



throw no light on the influence we are studying. In proportion as such cases are intermingled with others fitted to throw light on the subject, so will they obscure that light. Deaths in childbed from puerperal fever are, to some small extent, truly described as accidental : no cause for the supervention of the disease may be detected ; just as this is the case, so will be the value of the testimony of such accidents to the influence of the number of the pregnancy.

Before discussing generally the influence of the number of the different successive pregnancies, I shall compare, first of all, the influence of primiparity as compared with that of births after all subsequent pregnancies. It is well known that first pregnancies are, as a whole, attended by a much greater mortality than subsequent pregnancies, and this is a circumstance which scarcely demands explanation, for the primiparous woman has a longer and more difficult labour than others ; many primiparæ are delivered under the influence of depressing mental emotions ; in primiparous women all the arrangements, mechanical and other, for delivery are tested, and subsequent deliveries occur only in those who have so far successfully endured the trial as to survive. But it is particularly illustrative of the topic of this paper to inquire what effect primiparity has in labours that are natural, in women against whose chances of recovery nothing is known, who have easily passed through their trials.

Drs. Johnston and Sinclair, in their valuable work on *Practical Midwifery*, describe 11,874 cases in



which the labours were "purely natural." Of these 3699 were examples of primiparity. There were, therefore, 8175 births after the first. Of the 3699 primiparæ, whose labours were purely natural, 20 died of puerperal fever. Of the 8175 natural deliveries in women who had already passed safely through the dangers of parturition, 21 were followed by puerperal fever and death. To compare these proportionally, among purely natural deliveries in primiparæ, every 185th woman died of puerperal fever, or  $\cdot 57$  per cent; while among similar deliveries in multiparæ only every 389th woman died, or  $\cdot 25$  per cent. Puerperal fever deaths are described by Messrs. Johnston and Sinclair as "considered accidental." Their interesting data show that primiparæ are very greatly more liable to this awful accident than others.

In order to illustrate this particular point I have no other collection of cases of natural labour to refer to, and therefore nothing so valuable and directly applicable. But I shall adduce evidence derived from more general collections of cases, including all kinds of labour.

Professor Hugenberg of St Petersburg has published some observations on this point made in hospital practice.\* Of 2253 primiparæ 97 died of puerperal

\* "Das Puerperalfieber im St. Petersburger Hebammen-Institute von 1845-1859." S. 24. Separat-Abdruck aus der *St. Petersburger Medicinischen Zeitschrift*. For some further statistics pertinent to the question, see the *Klinik der Geburtskunde*. Von Dr. C. Hecker und Dr. L. Buhl. S. 226.



fever. Of 5783 multiparæ 141 died of the same class of diseases. Among the primiparæ puerperal fever death seized every 23d woman, or 4·35 per cent; while among the multiparæ death in the same form seized only every 40th woman, or 2·44 per cent.

Dr. Collins in his *Practical Treatise* describes 56 deaths from puerperal fever. Of these, 30 occurred among 4969 primiparæ, and 26 occurred among 11,445 multiparæ. Among the primiparæ every 165th woman died of this disease, or ·6 per cent. Among the multiparæ every 440th woman died of it, or ·23 per cent.

Among the married women whose deliveries were registered in Edinburgh and Glasgow in 1855 there were 58 puerperal fever deaths. Of these, 26 occurred in 3722 primiparæ, and 32 in 12,671 multiparæ. Of the primiparæ puerperal fever carried off every 143d, or ·7 per cent; of the multiparæ every 396th, or ·25 per cent.

Having shown, by the statistics already brought forward, that deaths from puerperal fever are among primiparæ at least twice as numerous as among multiparæ, I proceed now to inquire into the comparative mortality from this cause in labours following succeeding pregnancies.

Hugenberger devotes a short paragraph to this topic, and gives interesting data, which I here produce in a tabular form:—



TABLE XCII.—SHOWING THE MORTALITY FROM PUERPERAL FEVER IN DIFFERENT PREGNANCIES. (FROM HUGENBERGER.)

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage of Deaths.	Or one in
1st . . .	2253	97	4.30	23
2d to 4th .	4031	85	2.11	47
5th to 9th	1563	47	3.01	33
10th to 19th	189	9	4.76	21

This table of Hugenberg's data justifies his remarks. He says that the greater or less frequency of previous pregnancies appears to be not without influence upon the lying-in, for while those pregnant from the second to the fourth time show the most favourable results, the first increment of mortality begins with those in the fifth to the ninth pregnancies; and a greater mortality still is observed in women in the tenth to the nineteenth pregnancies. If we could dare to adopt as demonstrated what Hugenberg's data seem to show—and as yet I have adduced nothing calculated to shake their evidence—we should have an extremely interesting addition to our knowledge of the influence of the number of the pregnancy upon the danger of confinement. It would appear that from the very great danger of a first confinement, woman passed into a period of comparative safety in the next succeeding confinements, till she came to about the fifth lying-in, when danger began to increase; and as pregnancy succeeded pregnancy, danger still further



increased, until it reached a degree as great as that of a first confinement.

An interesting contrast of these results with what is known of the fecundity of women at different ages may be made. The average age of wives in Edinburgh and Glasgow bearing first children is 24 years. The average age of wives bearing fifth children is 31 years. From the 25th to the 30th year women are more fertile than at any other time. It is within the ages of 25 to 30 that are included the average ages of women bearing second, third, and fourth children, those produced with least danger to life. Hence, if the data are good and sufficient, there is a coincidence between the time of the greatest amount of safety and that of the greatest fecundity ; and diminished fecundity, or likelihood of having children, occurs when danger is great ; that is, in first pregnancies and in fifth and subsequent pregnancies, or in pregnancies of women below 25 years of age and above 30. But this point will be better and more directly demonstrated when the influence of age is itself discussed.

I shall now bring forward other data similar to Hugenberger's, with a view to observing whether they confirm his results or not.

In Edinburgh and Glasgow in 1855 there were 58 puerperal fever deaths of wives, all occurring before the ninth pregnancy. There were in that year delivered 15,384 wives pregnant for the eighth time or less. Arranging these according to the number of the pregnancy, we have the following :—



TABLE XCIII.—SHOWING THE PUERPERAL FEVER DEATHS OF  
WIVES DELIVERED IN EDINBURGH AND GLASGOW IN 1855.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage of Deaths.	Or one in
1	3722	26	·698	143
2	2893	8	·276	361
3	2534	11	·434	230
4	1982	6	·303	330
5	1543	2	·129	721
6	1221	2	·164	610
7	848	1	·118	848
8	641	2	·312	320

This table is scarcely a fit object of comparison with Hugenberg's, for it will be observed that while his table has cases of death in women even in the nineteenth pregnancy, no wife died after delivery in Edinburgh and Glasgow in 1855 whose pregnancy was above the eighth. So far as this imperfect table goes, however, it is somewhat in opposition to the general tenor of the results published by Hugenberg.

In the work on practical midwifery by Johnston and Sinclair I find a table of 75 puerperal fever deaths, in 74 of which the number of the pregnancy is given. Unfortunately, I can discover in the work no data regarding the number of the pregnancies of the whole women delivered. Unwilling, however, to lose any advantage that may be gained from the table of pregnancies of 74 puerperal fever deaths, I have in the following table arranged them for comparison with the



TABLE XCIV. — SHOWING A COMPARISON OF PUERPERAL FEVER DEATHS IN THE DUBLIN HOSPITAL WITH THE NUMBER OF PARTURIENT WIVES IN EDINBURGH AND GLASGOW in 1855.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage of Deaths.	Or one in
1	3722	40	1·07	93
2	2893	6	·27	482
3	2534	11	·43	230
4	1982	3	·15	661
5	1543	3	·19	514
6	1221	4	·32	307
7	848	2	·23	424
8	641	0		
9	425	3	·70	141
10	222	1	·45	222
11	152	1	·60	152

whole women delivered in Edinburgh and Glasgow in 1855. Of course the percentages derived from this comparison are not figures of actual value, but only of value for comparison with one another; and it is interesting to observe that they roughly confirm the results of Hugenberger. After a great mortality in first pregnancies, there is a great improvement in second, third, and fourth pregnancies; and then, again, as the fifth pregnancy is passed, the mortality rises as the number of the pregnancy increases. It must be admitted that this accordance is not very exact, the regularity of the results being disturbed by the great mortality in third pregnancies, and the absence of



mortality in eighth pregnancies. There can be no doubt that the value of the table is not very great; yet it evidently points towards confirmation of Hugenberg. Larger and better data are required to produce a satisfactory assurance.



## CHAPTER II.

THE RELATION OF THE NUMBER OF THE LABOUR TO THE  
MORTALITY ACCOMPANYING PARTURITION.

IN pursuing this topic I shall follow the same course as I observed in describing the mortality from puerperal fever, beginning by a comparison of the mortality of first labours with that of all subsequent labours.

The first data which I adduce are Johnston and Sinclair's 11,874 cases of purely natural labour. These are specially valuable for the purpose, for nearly the very reasons which enhanced their value when puerperal fever was the only cause of mortality under consideration—reasons which, therefore, need not be repeated here. Of 11,874 purely natural cases, 3699 were first labours, and 8175 subsequent labours. Of the primiparæ 33 died, and of the multiparæ 34, or 1 in 112 of the former, or  $\cdot 89$  per cent, and only 1 in 240 of the latter, or  $\cdot 41$  per cent.

These purely natural cases form part of a total of 13,748 labours described by Drs. Johnston and Sinclair. Of these, 4535 were primiparæ, and 9213 multiparæ. Among the former 83 died, or 1 in 54, or 1·8 per cent, and among the latter 80 died, or 1 in



115, or ·86 per cent, a mortality, it is to be remarked, relatively almost the same as among the purely natural labours.

We may now take those labours alone which were not purely natural. Of these, 836 were in primiparæ, and of these 50 died, being 1 in 17, or 5·98 per cent; while 1037 were among multiparæ, and of these 46 died, being 1 in 22, or 4·43 per cent. Here it is at once observed that the relative mortalities are nearly alike, forming a striking contrast to the relative mortalities under any other circumstances. It is unfortunate that this striking observation is founded on so few data. It cannot fail to excite reflections in the practical obstetrician. Such would be out of place in this book, and I shall only diverge so far as to remark that here the primiparæ evidently hold such a greatly improved position, that while in natural labour puerperal fever carries off proportionally twice as many primiparæ as multiparæ, and that while in labours generally, twice as many of primiparæ die as of multiparæ; yet in unnatural labours the balance is restored, the primiparæ escaping the special danger conjoined to all the evils connected with primiparity, nearly as often as multiparæ escape the special danger alone, without the evils which all the statistics hitherto adduced show to attend primiparity.

In Dr. Collins' *Practical Treatise* the deliveries of 16,414 women are described. Of these, 4969 were primiparæ, and 11,445 were multiparæ. Among the whole there occurred 164 deaths, but the number of



the pregnancy is given only in 160 cases. Of these 160 deaths, 80 occurred among the primiparæ, being 1 in 62, or 1·61 per cent; and 74 occurred among the multiparæ, being 1 in 155, or ·64 per cent.

In the work of Messrs. Hardy and M'Clintock on Midwifery and Puerperal Diseases, 6635 cases of delivery are described. Of these, 5852 are described as natural deliveries. Among them were 1752 first labours, and 4100 subsequent labours. In the former the deaths were 7, being 1 in 250, or ·4 per cent; in the latter 9, being 1 in 455, or ·22 per cent.

The whole cases in the work of M'Clintock and Hardy are, as already said, 6635. Of these 2125 were in primiparous women, and 35 died, being 1 in 60, or 1·65 per cent. Among multiparæ were 4510 deliveries and 30 deaths, being 1 in 150, or ·66 per cent.

In Edinburgh and Glasgow in 1855 there were 16,393 deliveries of married women. Of these, 3722 were in first labours, and 50 died within six weeks after delivery, being 1 in 74, or 1·34 per cent. The multiparæ numbered 12,671, and of these 103 died, being 1 in 123, or ·81 per cent.

Having thus compared the mortality of primiparæ with that of all other parturient women, I proceed to inquire into the mortality of each successive pregnancy.

The accompanying table is made from the Edinburgh and Glasgow registers for 1855. It exhibits the number of wives delivered in each successive pregnancy, their mortality, and the percentage of mortality to deliveries. Casting the eye along the



percentage column of this table, one does not discover any marked indication of a regular variation after the great mortality of primiparæ is passed.

TABLE XCV.—SHOWING THE MORTALITY AMONG WIVES DELIVERED IN EDINBURGH AND GLASGOW IN 1855, IN EACH SUCCESSIVE PREGNANCY.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage.	Or 1 in
1	3722	50	1·343	74
2	2893	24	·829	120
3	2534	25	·986	101
4	1982	13	·655	152
5	1543	13	·842	119
6	1221	7	·573	174
7	848	7	·825	121
8	641	8	1·248	80
9	425	3	·706	142
10	222	1	·450	222
11	152	1	·658	152
12	61			
13	34			
14	11			
15	6	1	16·666	6

I have no other similar exact data to add to what is given in the last table (XCV.) The authors from whom I derive the following data as to the pregnancies of women dying after delivery do not give the numbers of the pregnancies of all their cases with which to compare the pregnancies of those that died. But I here make the data regarding deaths available by comparing them with the pregnancies of the whole



TABLE XCVI.—SHOWING A COMPARATIVE PERCENTAGE OF DEATHS IN SUCCESSIVE PREGNANCIES.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage.	Or 1 in
1	3722	254	6·82	15
2	2893	60	2·07	48
3	2534	64	2·52	39
4	1982	39	1·97	51
5	1543	31	2·01	49
6	1221	28	2·29	43
7	848	16	1·88	53
8	641	15	2·34	42
9	425	13	3·06	32
10	222	9	4·05	24
11	152	5	3·28	30
12	61	1	1·64	61
13	34	4	11·77	8
14	11			
15	6	1	16·66	6

wives delivered in Edinburgh and Glasgow in 1855. This composite table will thus not give results or percentages representing actual values, but only results for mutual comparison, and I venture to think they are valuable. The table is prepared as follows:—The first column states the number of the pregnancy; the second gives the number of wives delivered in Edinburgh and Glasgow in 1855, in each successive pregnancy; the third column gives the number in each successive pregnancy of a collected mass of cases of childbirth deaths; the fourth column gives the percentages of these deaths in the deliveries in each successive pregnancy. In the third column



are given 540 deaths gathered with care from the following sources:—153 from the Edinburgh and Glasgow registers above referred to; 160 from a table in page 364 of the *Practical Treatise* of Dr. Collins; 162 from the *Practical Midwifery* of Drs. Johnston and Sinclair; and 65 from the work on *Midwifery and Puerperal Diseases* of Drs. M'Clintock and Hardy.

This last table appears to me to show with considerable force that, after a woman has passed her ninth pregnancy or thereabout, she comes gradually into more perilous child-bearing, danger increasing with every unit added to the number of her children. To collate with this, it is interesting to the obstetrician to note what has been already shown, that, after the ninth, pregnancies recur with greater rapidity than before it.

Having now led all the evidence I intend to adduce, I shall, in conclusion, add a few general and recapitulatory remarks.

First of all, it must be noted that I have, hitherto at least, said nothing regarding the nature of the relation between the number of the delivery and the mortality attending it. It is true the data recorded demonstrate more or less completely certain coincidences, which may be called laws. But they establish nothing further. These laws are as follows:—

1. The mortality of first labours is about twice the mortality of all subsequent labours taken together.
2. The mortality from puerperal fever following



first labours is about twice the mortality from puerperal fever following all subsequent labours taken together.

3. As the number of a woman's labour increases above nine, the risk of death following labour increases with the number.

4. As the number of a woman's labour increases above nine, the risk of death from puerperal fever following labour increases with the number.

5. If a woman have a large family, she escapes extraordinary risk in surviving her first labour, to come again into extraordinary and increasing risk as she bears her ninth and subsequent children.

These laws, although they merely state coincidences, have very important practical bearings, which are too self-evident to require description. They have also important pathological bearings, which were alluded to in the commencement of this part. The most important, perhaps, of these relate to puerperal fever. These also I shall not enter upon further than to say that the occurrence of puerperal fever specially among primiparæ, and women who have borne large families,—its pretty close correspondence in relative amount to the general mortality of parturition after different pregnancies,—its subjection also to the law of the duration of labour,—do not appear to me to lend support to the views hitherto generally entertained regarding it, and expressed in the words accidental, fever, contagious, epidemic. Another point under this head I shall merely mention. Authors,



comparing the mortalities of lying-in institutions, whether from puerperal fever or from other causes, are frequently found neglecting to begin by ascertaining whether or not they are fit objects of comparison, and under this head, *inter alia*, neglecting to ascertain the comparative amount of primiparity in each institution. It is plain that, unless there be nearly the same comparative amount of primiparity in the institutions, their respective gross mortalities cannot be justly contrasted with one another.

The well-known protraction of labour in primiparæ may to some appear a sufficient cause of the increased mortality of first child-bearing. But mere prolongation of labour for a few hours cannot, in my opinion, be regarded as any satisfactory explanation of the causation of this increased mortality. In one set of Johnston and Sinclair's cases the labours of primiparæ are called purely natural, and they are compared with similar 'purely natural' cases in multiparæ; and the mere addition of a few hours to the length of labour in such primiparæ is not a sufficient cause of their mortality being twice as great as that of similar multiparæ. Denman alludes to "a vulgar and pernicious error which makes no distinction between the slowness and the danger of a labour." It would be to fall into this error to explain the increase of mortality merely by increased length of labour.

It must be held as proved, that according as labour increases in length, so the mortality accompanying it increases; and that this is true not only of the whole



mortality, but also of the special mortality from puerperal fever. This law, although it must have weighty bearings on the mortality of primiparæ with their long labours, cannot be regarded as to any great degree throwing light on it; for we find new increments of mortality after the ninth labour, when we have no reason to believe that labour is more prolonged than in labours preceding the ninth, in which the mortality is less. In other words, we have the number of the labour denoting increase of mortality where there is no evidence of accompanying increase of its duration. The law of duration, then, does not enable us to explain the variations of mortality in different labours.

To completely exclude the influence of the law of duration would be very desirable; but we see no present prospect of doing this, except by processes of reasoning. Without such, it could only be done by comparing a series of labours of different number, but in all which the duration was the same.

It must be remarked that the law of duration certainly has important bearings on the data and arguments herein adduced to show the influence of the number of the labour, and that the extent of these bearings is undecided. At the same time it is equally sure that the law of the number of the pregnancy has important bearings on the data and arguments adduced to show the influence of the duration of labour, and the extent of these bearings is undecided. The mutual influence of the data and arguments in these demonstrations must be great, and it



remains for future observers to accumulate materials for either showing the amount of these influences, or for a separate demonstration of the laws by data which do not intermingle them in their conditions.

It is worth while to remark that, restricting for a moment our regard to the great mortality of primiparæ (exceeding as it does that of multiparæ, taken together), we have a set of cases—those of Johnston and Sinclair—where the deaths were from puerperal fever, and in which the average excess of duration of labour in primiparæ above that observed in multiparæ was 4 hours. In multiparæ the average duration was 8 hours; in primiparæ 12 hours. Looking at this increased duration, and the correspondingly increased mortality in primiparæ, with the light thrown on it by tables published by various authors to demonstrate the law of duration, it appears to me that the increase of mortality in primiparæ is above that which these tables appear to give as the increase corresponding to a rise in duration from an 8-hours' labour to a 12-hours' labour.

These various remarks I have made with a view to keeping the demonstration of the influence of the number of labour on childbed mortality in its proper light, to keep it separate from other laws or supposed laws with which it may be confounded. I have alluded, with this view, to the causation of the variations of mortality according to the number of the pregnancy. It is no main part of this paper to enter on this subject, but a few words may not be out of place.



It would be foolish to imagine that any injurious influence or the reverse could spring from the mere number of the pregnancy. A woman in a first may, and often does, have as fortunate a delivery as in any other. To ascribe to the number of pregnancy any potency would be to fall into the error of those students of the duration of labour who ascribe great potency to the mere addition of length to a labour. In the case of the law demonstrated in this part, and in the case also of the law of the duration of labour, it appears to me that the source of the variations of mortality is to be looked for in the introduction of complications. I here use the word complications in a much wider sense than is generally ascribed to it, wishing it to imply injuries or injurious tendencies far slighter than those ordinarily classed as complications of labour. I have no doubt that all of these, however minute or slight, have their weight in giving proclivity to a fatal termination of the childbed. Puerperal fever may have its root in an otherwise insignificant perineal laceration as well as in a phlebitis or endometritis.

In primiparæ, as labour goes on, complications occur which are not nearly so liable to attack a woman in her next subsequent labours. These have their origin in various sources, chiefly in mechanical difficulties, and these often so slight as not to take the case from the category of purely natural, in an arrangement where the labour is alone taken into consideration, to the exclusion of the childbed.



Multiparæ are specially and increasingly liable to complications of a different kind connected with constitutional diseases, and with local infirmities of the uterus.

This introduction of complications forms also the main explanation of the law of the duration of labour. Indeed, in a rough way, it may be held that the statement of duration is a statement of the increase of complications ; for it is known that as labour lengthens out, so complications increase in frequency. Without these complications duration would be of small importance, as the profession has generally held. Their introduction is present evil and the seed of future disasters. Tables have been framed to show the increasing introduction of complications as labour is prolonged, but I only refer to them. They are utterly useless, so far as I know them, because they are founded only on an enumeration of those of the graver sort. Further, the introduction of complications is not ruled exclusively by the duration of labour. Many are rather connected with precipitate parturition. The complications which probably contribute largely to produce the increased fatality of labours after the ninth are not all included, or capable of inclusion, in any statement of duration, being present before and after the process.



## CHAPTER III.

THE INFLUENCE OF CHILDBED MORTALITY, AND SPECIALLY OF THE MORTALITY CONSEQUENT ON PRIMIPARITY, ON THE WHOLE MORTALITY OF WOMEN AT THE CHILD-BEARING AGES.

BEFORE leaving the subject of the great danger of primiparity, I have pleasure in referring to and quoting Dr. Stark's remarks on the reciprocal influence of the mortality of primiparæ on the whole mortality of married women, as compared with unmarried women. In the part quoted it will also be noticed that Dr. Stark arrives, by a method of his own, at the conclusion that primiparity is specially a cause of death in lying-in women. He also devotes some remarks to the influence of immaturity of the female (15 to 20 years of age) in increasing the mortality of childbed.

“When,” says the author, “the proportional death-rates at the quinquennial periods of life are compared, it is seen that the high death-rate of the married female appears to be confined to the ages under 30 years; but that from 30 to 35, and again from 35 to 40 years, the death-rate of the married female falls below that of the unmarried.



“But the striking fact is, that the mortality of the married females between the ages of 15 and 20 years is higher than that of the married females even during the next three quinquennial ages—viz. it is higher than the married females at the ages of 20 to 25, 25 to 30, and 30 to 35 years. This is very remarkable, seeing that the mortality, as a general rule, increases with age; and the fact brought to light by the table, that the mortality of the married women between 15 and 20 years of age is much greater than at the three immediately higher quinquennial ages, seems to indicate that marrying before the frame has acquired its full development causes the woman to run greater risk of her life than if marriage had been delayed till the full growth was completed.

“But it is between 15 and 20 years of age that the greatest difference occurs between the mortality of the married and that of the unmarried female. Thus, according to the table, it would appear that in every 100,000 married women at that age 983 deaths occurred; while in an equal number of unmarried women at the same ages only 691 deaths occurred. In other words, supposing married and unmarried were in equal numbers between 15 and 20 years of age, 10 married would die for every 7 unmarried.

“At the next quinquennial period of life—viz. between the ages of 20 and 25 years, it appears that in every 100,000 married women 910 deaths occurred; while in an equal number of unmarried women only 783 deaths took place. In other words, in equal num-



bers of each class from 20 to 25 years of age, 9 married women would die for every 8 unmarried women ; so that the difference was much less between the death-rates of the married and the unmarried at that age than at the previous or junior age.

“ From the 25th to the 30th year of life the difference in the death-rates of the married and the unmarried is slight, inasmuch as 928 deaths occurred in every 100,000 married women, while 903 deaths occurred in a like number of unmarried.

“ From the 30th year of life, the chances appear to be in favour of married life, inasmuch as the table shows that between 30 and 35 years of age only 927 deaths occurred in every 100,000 married women ; whereas 941 deaths occurred in a like number of unmarried women at the same ages.

“ The same is seen at the next age—viz. from 35 to 40 years of age ; for while only 1116 deaths occurred among every 100,000 married women, there were 1181 deaths in a like number of unmarried women at the same ages.

“ It seems to be unnecessary to compare the relative death-rates of the married and unmarried females above this age. Speaking generally, it may be said that at every age above 30 years, the mortality of the married female is lower than that of the unmarried, so that the higher mortality of the married female is confined to the ages under 30 years.

“ The important question for solution, therefore, is, What is the cause of the mortality of the married



female being so much higher than that of the unmarried at the three quinquennial ages, 15-20, 20-25, and 25-30 ? To aid us in such an inquiry some additional facts are required ; and they are so far supplied by tables which were published for Edinburgh and Glasgow in our Second Detailed Annual Report. From these tables it appears that in Edinburgh and Glasgow in 1855, of 16,573 mothers 9274 were under 30 years of age, and 7299 above that age. That is to say, that, speaking in a very general way, nearly as many women above 30 years as under that age bore children in these towns during 1855. Now, it may be assumed that the relative proportions of women under and above 30 years of age who annually bear children in Scotland are much the same ; let us therefore apply this knowledge to the solution of the above problem.

“ If nearly an equal number of women above and under 30 years of age bear children, and if it be found, as Table XXV. proves, that the mortality of the married female is only greater than that of the unmarried female below 30 years, while above that age fewer married females die than unmarried, the conclusion seems irresistible that the greater mortality of the married female under 30 years of age cannot be owing simply to child-bearing, because it does not increase the mortality of the married female above 30 years of age. But that table just as distinctly proves that it is some danger connected with married life, because the unmarried females under 30 years of age are not subject to it ; and that it is a danger which is



greatest during the earlier years of marriage, and which disappears as life advances. Every medical practitioner seeing these facts would at once suggest, as the explanation, that this additional danger during the earlier years of marriage is the birth of the first child. It is a well-known fact that the risk to the mother is far greater at the birth of her first child than at any subsequent delivery ; and it is extremely probable that the whole extra mortality of the married female under 30 years of age, over that of the unmarried at the same ages may be caused by the greater dangers which attend the delivery of the first child. This can almost be demonstrated to be the true explanation.

“ If we throw the tables above referred to, having reference to the ages of the mothers in Edinburgh and Glasgow in 1855, into another form, as here given, they will show us the number of mothers in these towns at each quinquennial period of life, the numbers who at each age bore their first child, and the proportion of mothers bearing their first child to every 100 mothers at each age. We have then the following interesting result :—



TABLE XCVII.

Ages of Mothers.	Total Number of Mothers.	Number of Mothers bearing their First Child.	Proportion of Mothers bearing their First Child to every 100 Mothers.
15–20 years	403	354	87·8
20–25 „	3814	1921	50·3
25–30 „	5057	1019	20·1
30–35 „	3943	331	8·3
35–40 „	2395	124	5·1
40 and above	961	32	3·3

“Between the ages of 15 and 20 years, 87 per cent of the mothers were confined with their first child. Between the ages of 20 and 25 years, 50 per cent of the mothers were confined with their first child. Between 25 and 30 years, only 20 per cent of the mothers were confined with their first child; and the proportion diminished to 8, 5, and 3 per cent at the three succeeding quinquennial periods of life.

“Assuming, therefore, that in all Scotland the proportion of mothers bearing their first child is at each age the same as in these towns, and comparing these proportions with those of the married women who died at each quinquennial period of life, we are almost driven to the conclusion that the excessive mortality in the married female, as compared with that of the unmarried at the same ages, is almost solely due to the superadded dangers which attend the birth of the first child. That, in fact, after the birth of her first child, the married female, even during the rest of her



child-bearing life, has an equal chance of life with the unmarried, and has a better life than the unmarried after she has passed her 30th year.”\*

\* *Seventh Detailed Annual Report*, p. xxxi. Consult also Boudin, *Geographie et Statistique Medicales*, tom. ii. p. 77.



## CHAPTER IV.

THE RELATION OF AGE TO THE MORTALITY FROM  
PUERPERAL FEVER.

ON this subject important information is to be found in a letter addressed by Dr. Farr to the English Registrar-General, and published in the appendix to that officer's seventeenth annual report :—

“ What (says Dr. Farr) is the danger of death by childbirth among women of different ages who bear children during the year ? This is a different question, which is of practical importance both in medical science and in the business of life-insurance. The defect in the English schedule, which, as yet, contains no column for the ages of the parents of the children registered, renders it impossible to answer this question with precision. It will, however, be useful to obtain an approximate answer ; and this we have been able to give by determining the probable proportion of women who bear children at each age, from the Swedish returns, and by applying the fraction expressive of this proportion to the English women living in 1851 at the corresponding age, the probable number of them who became mothers every year is determined. The total



number thus determined for the year 1851 is 609,845 ; while the actual average number of the births in the seven years by the returns was 603,045. It is thus evident that the estimate differs to no great extent from the facts, and it may be assumed that the births corrected for twins, triplets, and still-born children in England, would represent nearly 609,845 child-bearings.”\*

The following table, extracted from the data supplied by Dr. Farr, shows the mortality from puerperal fever in four decenniads :—

TABLE XCVIII.—SHOWING THE MORTALITY OF CHILD-BEARING WOMEN FROM PUERPERAL FEVER, IN ENGLAND, AT FOUR DIFFERENT AGES. (FARR.)

Ages . . . . .	15-24	25-34	35-44	45-54
Childbearing women . .	107,440	328,720	166,140	7545
Deaths from puerperal fever	298	486	256	12
Percentage . . . . .	·277	·148	·154	·163
Or 1 in every . . . . .	360	676	649	628

The large figures in this table give great value to the result, that while childbearing women aged from 15 to 25 do die of puerperal fever in a proportion far

\* I have been repeatedly consulted by the medical officers of insurance offices as to the proper conduct of cases of application of women for insurance who were childbearing or had the prospect of childbearing in future. Any advice I have hitherto given has been in very general terms. In this volume, however, there is now given a basis from which the actuary may calculate the answers to the



exceeding that of women at any other age, the child-bearing women aged from 25 to 35 are carried off by the same disease in the lowest proportion compared with all others. Puerperal fever mortality at its lowest among the lying-in aged from 25 to 35, rises on either side of this age, but it rises far more quickly and highly as age decreases than as age advances.

It would be unphilosophical to draw from this table even a presumption as to the influence of age on puerperal mortality, until careful consideration has been made of all the influences besides age which may have a bearing on it. Now, as far as I know, the paramount influence interfering with deductions from this table as to the influence of age is that of the number of the labour. Of the influence of primiparity, Dr. Farr, Dr. Tyler Smith (*Manual of Obstetrics*, chap. xlviii.), and Dr. Stark, have had some degree of appreciation. But Dr. Hugenberg has, in some data he has published, actually separated the primiparous from the multiparous, with the view of eliminating this great influence. I here produce the tables of Hugenberg, re-arranged for uniformity's sake (XCIX. and C.)

These tables are interesting, and seem to show that Hugenberg felt the necessity, in the study of the bearing of age on puerperal fever mortality, of separating most important questions in this topic. He can determine the fecundity of the female, or her chance of having offspring; the fertility, or the number she is likely to have; the time when she will probably become relatively sterile; the risk of death in bearing her first child; and, if she survives the birth of her first child, the risk of death in her subsequent confinements.



TABLE XCIX.—SHOWING THE MORTALITY OF PRIMIPARÆ, OF DIFFERENT AGES, FROM PUERPERAL FEVER, IN THE MIDWIVES' INSTITUTE OF ST. PETERSBURG. (HUGENBERGER.)

Ages . . . . .	15-18	19-22	23-26	27-35	36-45
Childbearing women . .	147	859	711	495	41
Deaths from puerperal fever	7	25	22	39	4
Percentage . . . . .	4·76	2·91	3·09	7·88	9·75
Or 1 in every . . . . .	21	34	32	13	10

TABLE C.—SHOWING THE MORTALITY OF MULTIPARÆ OF DIFFERENT AGES, FROM PUERPERAL FEVER, IN THE MIDWIVES' INSTITUTE OF ST. PETERSBURG. (HUGENBERGER.)

Ages . . . . .	18-22	23-26	27-35	36-53
Childbearing women . .	503	1410	2967	903
Deaths from puerperal fever	11	29	74	27
Percentage . . . . .	2·18	2·05	2·49	2·99
Or 1 in every . . . . .	46	48	40	33

ing primiparæ from multiparæ. Any special results which might be drawn from them I think little worthy of consideration, in deference to the much larger and more valuable data which I adduce, and on account of the extraordinary mortality which the tables reveal.

It may with truth be said that, to make a perfectly satisfactory comparison of the mortalities of women of different ages, it is necessary to compare with one another masses of women of different ages in each successive pregnancy. I know of no data for this



purpose. Hugenberg's data of primiparæ are a poor instalment, and my own Edinburgh and Glasgow data are equally insufficient, and I do not think it necessary to encumber these pages with them.

I have, however, ventured to increase the value of Farr's data, with a view to the question of the influence of age, by the following method. In Table CI. the result is given.

The correction for primiparity is made because the puerperal fever mortality after first labours is at least double the puerperal fever mortality of all other

TABLE CI.—SHOWING THE MORTALITY OF CHILDBEARING WOMEN FROM PUERPERAL FEVER IN ENGLAND, AT FOUR DIFFERENT AGES, CORRECTED FOR PRIMIPARITY.\*

Ages . . . . .	15-24	25-34	35-44	45-54
Childbearing women	107,440	328,720	166,140	7545
Deaths from puerperal fever corrected for primiparity . . . . }	194½	339½	256	12
Percentage . . . .	·181	·121	·154	·159
Or 1 in every . . .	552	823	649	629

\* It is to be remarked that this and the following tables corrected from Farr's data give results for different decennials that may be compared only with one another. The table would not give actual values even were Farr's data actual values, which they are not. Correction has been made only in the line of deaths by



labours taken together. In order to remove entirely, or almost entirely, the disturbing influence of primiparity, then, it is necessary to turn out of the data one-half of the deaths of primiparæ. The number of puerperal fever deaths of primiparæ at different ages is got by determining their probable proportion from the Edinburgh and Glasgow mortality of 1855.\* The preponderance of primiparæ at the earlier ages renders this correction necessary, and I only regret that the smallness of the data prevents us from ascribing to the correction a high value.

It may with truth and with some cogency be said that Farr's table should be further corrected for the increased mortality accompanying ninth and subsequent pregnancies which fall into the more advanced ages. I do not attempt this correction, because it

taking away one-half of the deaths of primiparæ. This makes the table read as if a table of multiparæ. This proceeding, being simpler, has been preferred to another, which might have been followed—namely, to extract from the mothers the whole primiparous by estimate, and to extract from the deaths those of primiparæ, and compare the remaining multiparæ and deaths of multiparæ.

\* TABLE CII.—SHOWING THE MORTALITY FROM PUERPERAL FEVER OF PRIMIPARÆ IN EDINBURGH AND GLASGOW IN 1855.

Ages . . . . .	15-19	20-24	25-29	30-34	35-39	40-44
No. of primiparæ . . . . .	331	1859	1007	354	134	33
Deaths by puerperal fever .	2	14	7	3		
Percentage . . . . .	·604	·753	·695	·847		
Or 1 in . . . . .	165	133	144	118		



cannot, with the means at my disposal, be done satisfactorily. But the omission of this correction will, comparatively, cause little inaccuracy in the results drawn from the table; for births in ninth and subsequent pregnancies are proportionally few, and the average age of women in ninth and subsequent pregnancies is above thirty-seven years, an age before which the injurious influence of elderliness appears to have already shown itself.\*

\* TABLE CIII.—SHOWING THE NUMBER OF CHILDREN BORN IN FIRST AND SUBSEQUENT PREGNANCIES IN EDINBURGH AND GLASGOW IN 1855, AND THE AVERAGE AGES OF THE MOTHERS IN EACH SUCCESSIVE PREGNANCY.

No. of Pregnancy.	No. of Children.	Average Age of Mother.
1st	3722	24·6
2d	2893	26·2
3d	2534	27·6
4th	1982	29·9
5th	1543	31·5
6th	1221	32·9
7th	848	34·9
8th	641	36·1
9th	425	37·5
10th	222	38·8
11th	152	39·2
12th	61	40·0
13th	34	41·7
14th	11	42·4
15th	6	42·7
16th	2	48·5
17th	2	41·5
18th	1	40·0
19th	1	48·0



It has, lastly, only to be remarked that reference to Table CI., corrected, as it is, for primiparity, shows results still closely resembling in general features those derived from the uncorrected table. Though similar in general features, there is evidently great difference in the numerical variations in the two Tables (XCVIII. and CI.), and I think there can be no doubt that the last table (CI.) gives an approximation to a view of the influence of age far more faithful than the first (XCVIII.)



## CHAPTER V.

THE RELATION OF THE AGE OF THE MOTHER TO THE  
MORTALITY ACCOMPANYING PARTURITION.

THE first table which I shall adduce under this head is extracted from the data of Dr. Farr, already referred to. The calculations, as made by Dr. Farr, give the mortality according to age, but, since primiparous females are included in the lists, they are of little value as indicating the influence of age. I have, as in the table of puerperal fever deaths, corrected Dr. Farr's data for primiparity, and in the penultimate line given the percentages; which may be held as showing, when compared one with another, an approximate estimate of the influence of age on the mortality of parturition.



TABLE CIV. — SHOWING THE MORTALITY OF CHILDBEARING WOMEN IN ENGLAND AT FOUR DIFFERENT AGES, AND THE SAME CORRECTED FOR PRIMIPARITY.

Ages . . . . .	15-24	25-34	35-44	45-54
Childbearing women	107,440	328,720	166,140	7545
Deaths . . . . .	718	1397	1051	66
Percentage . . . . .	·668	·425	·633	·883
Deaths corrected for } primiparity . . . . .	473	1216	1033	66
Percentage . . . . .	·440	·369	·621	·875
Or 1 in every . . . . .	227	270	160	114

Here the large figures give a proportionate value to the results. The women aged from 25 to 34 have the fewest deaths among them—namely, ·369 per cent. They are more fortunate than the very young women aged from 15 to 24 by ·07 per cent. But the seeming influence of this youthfulness in aggravating the danger of women is slight when compared with that of advancing years, the decennials following that from 25 to 34 showing a mortality increasing in a far higher ratio.

The next table which I adduce is made from data furnished by Dr. Collins' *Practical Treatise*.



TABLE CV.—SHOWING THE MORTALITY OF CHILD-BEARING WOMEN IN THE DUBLIN HOSPITAL  
DURING DR. COLLINS' MASTERSHIP.

Ages . . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54	Total.
No. of Mothers . . . . .	762	4862	5309	3817	1210	397	22	6	16,385
Deaths . . . . .	10	38	41	41	26	4	...	...	160
Percentage . . . . .	1.312	.781	.772	1.074	2.148	1.007	...	...	.976
Or 1 in every . . . . .	76	128	129	93	46	99	...	...	102



It is not corrected for primiparity, and I bring it forward for its own value, and because it gives the data for periods of five years. It will be observed that its results agree in the main with those derived from the data of Dr. Farr. But a further step is attained by the five-year divisions showing that youthfulness is influential chiefly below 20 years, and increasing age not until 30 years are passed. These results are, in my opinion, however, somewhat modified by the data which follow.

The next table (CVII.) contains only multiparæ. Primiparæ are excluded; there is therefore no correction to be made. Further, it is not estimated; the figures all show actual values.\*

It is worthy of remark that this table of actual values shows a minimum of mortality at the age of from 25 to 29 years.

The last table (CVIII.) to be given is a composite one, but appears to me to be of value with reference to the present question. It is made up as follows:—

\* To complete the view of the mortality of child-bearing women in Edinburgh and Glasgow in 1855, I here give the mortality of the primiparæ. It is not placed in the text because of the smallness of the figures compared with those of multiparæ, and because the table of multiparæ is more like the others in the text.

TABLE CVI.—SHOWING THE MORTALITY OF PRIMIPARÆ AT DIFFERENT AGES, IN EDINBURGH AND GLASGOW IN 1855.

Ages . . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	Total.
No. of Primiparæ	331	1859	1007	354	134	33	4	3722
Deaths of ditto .	4	24	13	8	1	...	...	50
Percentage . .	1·208	1·291	1·291	2·260	·746	...	...	1·343
Or 1 in every . .	83	77	77	44	134	...	...	74



TABLE CVII.—SHOWING THE MORTALITY OF MULTIPARÆ AT DIFFERENT AGES IN EDINBURGH  
AND GLASGOW IN 1855.

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45-49	Total.
Multiparæ	45	1829	4030	3496	2273	807	92	12,572
Deaths in ditto	...	13	21	39	20	8	2	103
Percentage	...	.711	.521	1.115	.879	.991	2.174	.819
Or 1 in every	...	140	192	89	113	100	46	122



TABLE CVIII.—SHOWING THE COMPARATIVE MORTALITY OF CHILDBEARING WOMEN AT  
DIFFERENT AGES.

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54
Mothers (Collins)	762	4862	5309	3817	1210	397	22	6
Deaths, corrected for primiparity	10½	59	89	91½	52¾	15	1	
Percentage	1·3	1·2	1·6	2·3	4·3	3·7	4·5	
Or 1 in every	72	82	60	42	23	26	22	



With the number of women confined at different ages in the Dublin Hospital under Dr. Collins, are compared the deaths at different ages recorded by Drs. Collins, M'Clintock and Hardy, and Johnston and Sinclair. These deaths have been collected with considerable care to insure a close approach to their true number. All the deaths were among women delivered in the same hospital, and these are compared with another set of cases also delivered in it. The deaths are corrected, as in former tables, for primiparity. The resulting percentages are of course of value only when compared with one another, and in this respect they appear to me to be very valuable. The smallest mortality is seen to be in the age 20-24 ; and the increased mortality from greater youthfulness is in the next quinquennial period less than the corresponding increase on the other side from greater age.

Looking over these tables, one cannot doubt that the result of Farr's data, showing 25 to 35 as the age of smallest mortality, may be considered as justly supplanted by the results of the tables showing quinquennial periods. In all of these the smallest mortality is found to be under 30 years of age. Of the tables showing quinquennial periods, given in the text, No. CVIII. gives 20-24 as the safest age for parturition, while the CVIIth gives it as 25 to 29 ; and we may guess with considerable assurance that the age of minimum mortality from parturition is at or near 25 years.



The following are the chief conclusions deducible from the whole exposition :—

1st, Youthfulness has less influence in producing mortality from parturition than elderliness.

2d, From the earliest age of child-bearing there is a climax of diminishing puerperal mortality, succeeded by an anti-climax of puerperal mortality increasing till the end of child-bearing life.

3d, The age of least mortality is near 25 years, and on either side of this age mortality gradually increases with the diminution or increase of age.

4th, Above 25 years puerperal mortality increases at a much higher rate than it increases at corresponding periods below 25 years, a circumstance which decidedly throws the greater safety to the side of the quinquenniad 20-25.

5th, Though it is not deducible from anything in this part, it is too interesting to escape notice that the age of greatest safety in parturition coincides with the age of greatest fecundity, and that during the whole of child-bearing life safety in parturition appears to be directly as fecundity, and *vice versa*. “To the female sex,” says Aristotle, “premature wedlock is peculiarly dangerous, since, in consequence of anticipating the demands of nature, many of them suffer greatly in childbirth, and many of them die.”\*

\* Sadler, *Law of Population*, vol. ii. p. 272.







## PART VIII.

### ON THE AGE OF NUBILITY.

IN this discussion I have nothing to say that is applicable to individual women. Such may, by peculiarities of constitution, be to a greater or less extent removed from subjection to the laws which govern the sex generally. Individuals may, with propriety, be advised to marry earlier than general laws would sanction, or later. My object is to point out the ages within which women generally should enter the married state, if they are to be guided by physiological laws. I shall hold it to be the object of marriage, as it is its natural result, to "multiply and replenish the earth." And I shall omit entirely from consideration at present those moral considerations bearing on this topic which may, in an important sense, be justly included under the designation of physiological laws.

It is, I believe, a common notion that the occurrence of menstruation indicates the arrival of the nubile age. Authors occasionally use such expressions as advent of nubility and commencement of menstruation as synonymous. The age of puberty may be contemporaneous with the age of nubility; but it



cannot be assumed to be so without proof, for very little reflection will suggest to the physiologist many reasons for supposing that the marriageable age is generally delayed for several years after the arrival of the age of puberty. And it is my object now to show at what age it is wisest for women to enter the married state.

M. Joulin\* makes some remarks which appear to me so just as to demand quotation here: "Nubility," says he, "is the complement of puberty. These two states should not be confounded; it is rarely that they are developed simultaneously, and their appearance is ordinarily separated by an interval of several years. Puberty is the age when the young girl becomes a woman; nubility the epoch when she may fulfil all the duties of maternity. It is frequent in our latitude to see the menses appear at eleven or twelve years; parturition will be, strictly speaking, possible, but after the young mother has escaped the dangers of a labour very painful to her, will she be able to nurse her child, to lavish on it the necessary attentions? will she be able to comprehend the whole extent of her task and to fulfil its laborious duties? No. For nubility is not arrived, and it is only then that the complete development of the organs, the powers, and the intelligence will permit her to be sufficient for the undertaking. The civil code authorises matrimonial union when the woman is fifteen years and the man eighteen; but civil law is not in accordance with physiological law,

\* *Traité Complet d'Accouch.* i. partie, p. 105.



and the race which would spring from unions so premature would soon proceed to degenerate. No general limit should be fixed, as is done, for nubility ; were this term 18, 20, or 22 years, numerous deviations from the common level would occur."

"When I am consulted," he adds, "as to the opportuneness of a marriage for subjects who are too young, I am accustomed to respond to the parents, that they should not marry their daughter—that is to say, expose her to the chance of becoming a mother—until for a year at least her stature has ceased to increase. This is the epoch that I fix for 'nubility ; embonpoint may add to the volume of the organs, but nature will add nothing to their development."

It is generally supposed, especially by cattle-breeders, and I believe justly, that incomplete development of the body indicates a certain unfitness for bearing young ; the too early performance of the function having an injurious influence upon the young mother, and resulting in offspring that is not generally excellent. In women the question of development is susceptible of more intimate examination than in any animal. For we have, recorded, careful inquiries into the growth of her physical frame that can be turned to account. I have already indicated that I do not here enter upon the important considerations of moral development in the female ; and in physical development I shall only condescend on the stature, the ossification of the pelvis, its development in shape, and the development of the genital organs.



The development of the genital organs seems to be one of completed growth in size and form, as early in life as marriage is ever contemplated in European countries, and therefore no restriction of the nubile age can be based on their state. I say "seems to be," for I know of no refined investigation of the anatomical details bearing on the subject. The statements of A. Farre,\* and of Kussmaul,† are not quite to the point ; and it can only be said that they imply completed growth at the age of puberty. But I may assume that few will maintain that the age of puberty and of nubility are identical ; and the mature state of the genital organs at early ages, even if demonstrated, therefore leaves the scope of this inquiry unrestricted.

"The full growth of man," says Quetelet, "does not appear to be attained at his twenty-fifth year."‡ Elsewhere the same author remarks that "the limits of growth in the two sexes are unequal : first, because woman is born smaller than man ; second, because she sooner finishes her complete development ; third, because the annual increase which she receives is smaller than that of man." These conclusions of Quetelet regarding the stature of woman are founded on a large collection of data of different kinds, and may be accepted as proof that women generally are

\* Article "Uterus," in Todd's *Cyclopædia of Anatomy and Physiology*.

† *Von dem Mangel, etc., der Gebärmutter*.

‡ *A Treatise on Man*, p. 61.



increasing in stature till at least about their twenty-fifth year of age, that till this age they are immature inasmuch as they are not full-grown in height.

The tardy ossification of the bones of the pelvis naturally attracts attention from their locality and inseparable connection with the function of reproduction. With the full details of the osseous growth and perfecting of the pelvic bones I shall not encumber this discussion. I shall merely refer the inquirer to anatomical works on the subject, and here make two quotations\* from the article "Pelvis," by John Wood, published in the *Cyclopædia of Anatomy and Physiology*:—"About the time of puberty, as first pointed out by M. Serres, a distinct complementary point of ossification appears in the cartilage dividing the bones in the cotyloid cavity. . . . According to Meckel, the pubis and ischium join first with each other, and the ilium becomes united to them afterwards. At the same time appear the four remaining complementary points as epiphyses. . . . All these are soldered to the bone about the twenty-fourth or twenty-fifth year, the epiphysis of the iliac crest being the last to join." Speaking of the sacrum, the same author remarks:—"At the age of sixteen years, the epiphysial or complementary ossific points begin to form—viz. on each articulating surface of the bodies of the sacral vertebræ is developed, as in the true vertebræ, a horizontal plate of bone, which, after

\* Vol. v. p. 120.



coalescing with the bodies to which they respectively belong, finally (except the first and last) become soldered to each other from below upwards, commencing with the two last vertebræ, at from the sixteenth to the eighteenth years, and completing the formation of the sacral bone by the union of the two first vertebræ, at from the twenty-fifth to the thirtieth years. Between the eighteenth and the twentieth years begins the formation, by scattered granules, of four lateral plates of bone—one on each side, forming the iliac articular surfaces, opposite to the three first vertebræ—and one on each side, opposite the two last. These unite with the sacral bone about the same time that its upper vertebræ coalesce.”\*

Having thus shown the lateness of the completion of the structural development of the pelvis as a bony skeleton, I advance to the still more important topic of the time of the complete construction in shape or form of the same part. It would be tedious and out of place here to show the obstetrical advantage and use of the special shape of the fully-grown female pelvis. I assume this.

“According to Dupuytren,” says Mr. Wood, in the article from which I have just been quoting, “the female pelvis differs very little from that of the male till puberty, at which period it has a general triangular form in both sexes, but after that period it becomes rapidly developed, and soon assumes its distinctive sexual character. The transverse diameters begin to

\* See also Litzmann, *Die Formen des Beckens*, s. 16.



exceed the conjugate, and, in the female, attain a great preponderance, constituting one of the great characteristics of the fully-formed human pelvis, as distinguished from that of the lower animals."

Burns offers us statements which are more apposite, and in distinct words gives them a bearing upon the question of nubility. The grand feature of his remarks, for our present purpose, is his proving that the female pelvis just before puberty, and perhaps so late as eighteen, is far from having assumed the form best suited for the difficulties implied in commencing maternity. "The shape," he says,\* "is different in the child and the adult. The dimensions of the brim are reversed in these two states; the long diameter of the foetal pelvis extending from the pubis to the sacrum. By slow degrees the shape changes. These changes, however, must be affected by the general growth of the body and the term of puberty. At nine years the conjugate diameter is two inches and seven-eighths, the lateral an eighth less; at ten years of age the antero-posterior diameter is three inches and a quarter, the lateral is an eighth more; at thirteen the former is still the same, but the latter has increased to three inches and three-quarters; at fourteen the former is three and three-quarters, the latter four inches. Just before puberty, perhaps so late as eighteen, the antero-posterior diameter is three inches and seven-eighths, the lateral four and a half. These measurements I give, however, from individual pelvises. If a girl

\* *Principles of Midwifery*, tenth edition, p. 23.



should very early become a mother, the shape of the pelvis may occasion a painful and tedious labour.” “To the female sex,” says Aristotle, “premature wedlock is peculiarly dangerous, since, in consequence of anticipating the demands of nature, many of them suffer greatly in childbirth, and many of them die.”\* Litzmann† has gone into this question with greater fulness than Burns, but I shall only extract from his work the observation that he has given the dimensions of two pelves of young women of nineteen years of age, and that in both the measurements distinctly indicate that they have not yet arrived at the average size and shape. They therefore confirm the statements of Burns on this point in every respect.

It is known that a first confinement is much more dangerous than any of those which follow, at least until the confinement reaches a number above that ordinarily attained to by fertile women. I have elsewhere shown that this extraordinary mortality accompanying first labours is about twice that accompanying all subsequent labours taken together.‡ It evidently, then, becomes of extreme importance for the young woman entering on the risk of a first confinement to do so at the most favourable age. The age of smallest mortality after a first confinement should be chosen for

\* *De Repub.* l. vii. c. 16. See Sadler, *Law of Population*, vol. ii. p. 272.

† *Die Formen des Beckens.*

‡ P. 241.



encountering its risks. It has been long known that age has considerable influence on this mortality. But I know of no satisfactory data for deciding at what age a woman most safely bears a first child. In another place.\* I have entered upon the subject, and shown that the quinquenniad 20 to 24 years inclusive is the safest for parturition generally, and I think it a natural inference that that age is the safest for a first parturition; an inference, too, which appears to me to have the support of the general tenor of the argument to which I have made reference. If a woman is to multiply and replenish the earth, as married women ordinarily do, she must survive her first confinement. To have the best chance of this survival she should marry between 20 and 25 years of age.

There is scarcely any condition of a married woman which more surely causes unhappiness than sterility; its avoidance is therefore a great object. If a married woman is sterile, she fails to secure the great end of the union. It is evident that the age of nubility should be fixed with a view to the securing of fecundity. I have elsewhere† shown that age at marriage has considerable influence upon the occurrence of sterility; and the age at marriage found to be most secure of fecundity is the quinquenniad 20 to 24 years inclusive. So far, then, as the avoidance of sterility has any bear-

\* Table CVIII. p. 273.

† *Transactions of Royal Society of Edinburgh* for 1866. "On some Laws of the Sterility of Wives," chap. iv. See also p. 193.



ing upon the age of nubility, the quinquenniad 20 to 24 years inclusive is to be selected.

“Premature conjunctions,” says Aristotle, “produce imperfect offspring, females rather than males, and these feeble in make and short in stature. That this happens in the human race as well as in other animals, is visible in the puny inhabitants of countries where early marriages prevail.”\* These opinions of Aristotle are confirmed, so far as stature is concerned, by my own researches and those of Professor Hecker, regarding the length and weight of children born of mothers of different ages.† And the statistics of Dr. A. Mitchell seem to show that immature mothers and old mothers are specially liable to bear idiot children.‡

If, in the foregoing paragraph, it is established or rendered probable that the children of very early marriages are less strong and healthy than other children, it may be considered a work of supererogation to show that such children die in a higher proportion in early life than others. But the demonstration of both of these points is not perfect, and the proof of the one goes far to confirm the other, and is therefore demanded.

Sadler, in his work on the *Law of Population*, enters upon the mortality of children as influenced by the age of the mother at marriage. His enthusiasm

\* *De Repub.* c. iv. p. 246, Gillies' translation. See Sadler, l. c. p. 273.

† *Edinburgh Medical Journal*, December 1864. See also p. 62.

‡ *Ibid.* January 1866. See also page 290.



for a preconceived theory diminishes the value of his remarks on the point ; and the circumstance, that his data do not give the results of marriages after 32 years of age of the woman, renders the whole of less value than a more extended series of data and calculations would possess.\* So far as they go, however, they show a diminishing mortality among the children in proportion as the age at marriage increases.

A more valuable collection of data is to be found in the report of an investigation into the state of the poorer classes of St. George's-in-the-East.† The following table, extracted from that document, gives the

TABLE CIX.

Years elapsed since birth of first child.	Mortality per cent of the Children born to Marriages formed at Ages			
	16-20	21-25	25-30	31-35
10	36·87	37·09	37·89	35·48
20	47·44	43·10	44·36	16·67
30	53·03	43·89	48·53	64·29
40	63·12	57·14	68·00	50·00

foundation of the conclusions arrived at, which I now give in the words of the report. “From this abstract it is obvious that, of the three first periods, the children born of marriages formed in the quinquennial term of life 21-25, are subject to a less rate of mortality than those of the period immediately preceding or imme-

\* *Law of Population*, vol. ii. Tables xlv. and xlvi.

† *Journal of Statistical Society*, xi. p. 223.



diately following; the rate of mortality in the most advanced period, 31-35, is very irregular, and no doubt arises from the very small number of families included in that group."

This interesting table, then, of the report cited, shows a greater survival of children born of women married at from 20 to 25 years of age than at any other; and as the rearing of children is assumed to be one of the chief objects of marriage, the age to be selected for marriage with a view to this object is 20 to 25 years.

Before leaving this point I must add the evidence of two gentlemen skilled in the breeding of lambs and of calves. They say that the mortality of the young of these animals, when the mothers are immature, is much greater than when they are well-grown. One of them says—"Taking the first lamb from ewes at one year old has in almost every case failed to be remunerative, owing to the frequent deaths of the lambs. The same may be said of young heifers, though the mortality of the offspring may not be so marked as in that of sheep."

Considering the argument drawn in this paper from the avoidance of sterility, it may appear to some to be unfair to found any argument upon the avoidance of an excessive family. And I admit that what I have to adduce on this subject may partake in some degree of the nature of an arbitrary assumption. Having, however, shown some grounds for believing that ten is the ordinary limit of fertility in women living in wedlock



during the whole child-bearing period,\* and having shown the very great mortality attendant upon confinements numbering the tenth, or higher,† I venture to express my belief that a family rising above ten begins to be excessive. Now it appears to me that all the knowledge we possess of the laws of fertility refers the excessively numerous families in a population to fertile women who have been prematurely married. Such women certainly go on longer bearing children than any others, counting up to the end of the child-bearing period in the women compared.

Another class of women is liable to have children with dangerous rapidity, and often a family that is excessive, at least when the duration of married life is taken into account—namely, those who are fertile when married comparatively late in life.‡

\* *Transactions of the Royal Society*, 1866, p. 292. See also p. 117.

† *Edinburgh Medical Journal*, September 1865. See also p. 247.

‡ I might have introduced into the text what I here subjoin in a note, had I regarded the opinions suggested in connection with twinning as very well ascertained. If the connection of twin-bearing with primiparity be, for the time, left out of view, there would remain little doubt upon the conclusions now to be suggested: Primiparity, involving, as it does, special danger to mother and to child, as well as to the latter liability to idiocy and other evils, demands study as a peculiar parturition distinguished from those which follow it. I have shown (see page 90) that twins increase in frequency as the number of the pregnancy increases: twin-bearing, then, is, in some sense, an indication of an excessive family. They specially often come to overburden a mother already overwhelmed



Child-bearing by an immature mother is popularly held to be dangerous to the continued general health of the mother, and to prevent her complete development with progeny. The children of advanced life and of pregnancies of high number Dr. A. Mitchell has shown to be more frequently idiots than other children. Twins (see page 66) are also specially liable to idiocy. The accumulated evils of an excessive family are thus apparent.

As the conclusions of Dr. A. Mitchell have a weighty bearing on this point, I here quote the relative part of his paper from the *Edinburgh Medical Journal*, January 1866 :—

*“ Comparative Frequency of Births of Idiots in First and Subsequent Pregnancies.*

“ Among 443 idiots and imbeciles consecutively examined, I found 138 first-born, or 31·1 per cent ; and 89 last-born, or 20·1 per cent.

“ When it was known, however, that almost every sixth idiot in Scotland was illegitimate (663 idiots and imbeciles giving 108 illegitimate, or 17·1 per cent), it was thought that an element of disturbance was probably thus introduced into the foregoing figures, which might affect their value. The great majority of illegitimate children are known to be *first-born* and *only* children ; while not a few of them are *last-born*, though the last of a small number of pregnancies—say of two or three. It was therefore thought desirable that a fresh series of observations should be made, excluding the illegitimate, and dealing only with those born in marriage. It was also thought well to confine these observations to those cases in which not more than one idiot occurred in a family, and in which the idiocy was noticed very soon after birth—that is, in which it was probably congenital. Further, no cases were accepted but those in which the mothers at the time of the inquiry had passed the age of child-bearing, though some of them I think were widows before that age was reached. All these restrictions made it difficult to obtain a large series of observations, and account for their number not exceeding 85—44 males and 41 females.



ment in size and beauty. I have no positive evidence to adduce in favour of this generally-entertained notion, which my own experience appears to me

“I sent my results in detail to Dr. Matthews Duncan, who kindly drew up for me the two tables (CX. and CXI.) embodying the facts in a way which makes their teaching apparent.

“TABLE CX.—SHOWING THE COMPARATIVE FREQUENCY OF BIRTHS OF IDIOTS AND OF ALL BIRTHS IN FIRST AND SUBSEQUENT PREGNANCIES.

No. of Pregnancy.	Percentage of <i>all</i> Births.	Percentage of Idiot Births.
1st	22·8	33·0
2d	17·7	18·8
3d	15·5	17·6
4th	12·1	2·4
5th	9·4	2·4
6th	7·4	2·4
7th	5·2	7·0
8th	3·9	3·5
9th	2·6	2·4
10th	1·3	7·0
11th	·9	3·5

“This table is read in this way:—Of *all* the children born in Edinburgh and Glasgow in 1855, 22·8 per cent were first pregnancies, while of the 85 idiots 33 per cent were first pregnancies, and so on.

“What the table appears to teach is briefly this:—That idiocy is more likely to occur among first and latest (7th to 11th) pregnancies than among others. This is substantially the same thing as was taught by the first inquiry, which included 443 cases, and in which all that was asked was whether the patient was first-born or last-born.

“*Age of Mother of Idiot at time of Birth of Idiot.*

“The same 85 cases are used in the following table which were used in Table CX.



to confirm.\* In its corroboration, however, I can adduce the ample experience of eminent breeders of the lower animals. I have had this opinion expressed to me, especially in regard to mares, cows, ewes, and bitches. “Experience,” says Sussmilch, “shows this in animals; as, for example, among great cattle, the cow which has a calf too young never comes to the size and strength which she otherwise would have done.” To this Sadler adds—“Of this principle Virgil was fully aware; hence he says,—

‘Sed non ulla magis vires industria firmat,  
Quam venerem et cœci stimulos avertere amoris,  
Sive boüm, sive est cui gratior usus equorum.’

“TABLE CXI.—SHOWING A COMPARATIVE PERCENTAGE OF THE CHILDREN BORN AT DIFFERENT AGES OF MOTHERS TO ALL CHILDREN BORN, AND OF THE IDIOTS BORN AT DIFFERENT AGES OF MOTHERS TO ALL IDIOTS BORN.

Age . . . . .	20-24	25-29	30-34	35-39	40-44	45-49
P. C. of all Children <sup>12</sup> {.	22·62	39·99	23·61	14·76	5·15	0·58
P. C. of Idiots . . . .	25·88	25·88	10·58	10·58	23·53	3·53

“This table is read thus :—Of all children born in Edinburgh and Glasgow in 1855, 22·6 per cent were born of mothers whose ages were from 20 to 24 years; while of the 85 idiots 25·8 per cent were born of mothers of corresponding ages; and so on.

“What we learn from the table is this :—That mothers under 24 years of age and above 35 are those more specially liable to have idiocy in their children.”

\* I wished at this point to make use of a valuable paper by Dr. Tuke of the Fife County Asylum, on Puerperal Insanity; but I find it is not easily adapted to my purpose, though it contains many details which, especially if further elaborated, may be turned to account on questions like that discussed in this part.



I might again appeal to the very same principle in the vegetable kingdom ; for instance, there is not a horticulturist who is not fully aware that premature fruition is injurious to the growth and future prolificness of all the fructiferous tribes in existence.”\*

In conclusion it is almost useless to add that I consider the age of about from 20 to 25 the nubile age of woman. The numerous facts and arguments I have adduced appear to me to bear out distinctly this conclusion. Below 20 years of age woman is immature ; she runs considerable risk of proving sterile ; and if she does bear a child, she runs a comparatively high risk of dying in childbed ; besides, her early marriage brings other disadvantages which need not be again enumerated. The woman above 25 years of age is mature, but to counterbalance this she encounters some greater risks than the very young wife’s, though of a similar nature.†

\* *Law of Population*, vol. ii. p. 275.

† “Without the sanction (says Major Graham) of the laws of physiology, or of common sense, a girl may—but in the present day rarely does—marry at the age of 12, a boy at the age of 14, under the existing laws of England ; but the consent of parents and guardians is required in certain cases when either party has not attained the age of 21 ; and the proportional number of either boys or girls who marry under the age of 20 is happily small.”

“The 26th year (he adds in another place) is the mean age at which men marry, and the 25th year the mean age at which women marry in England and Wales. About this period of life the growth of man is completed. Half of the husbands and of the wives are married at the years of age 21 and under 25 ; the higher average is the result of later marriages, which occur in great numbers at the age of 25-30.

“Plato laid it down in his *Republic* that the men should be



united about the age 30-55, the women at the age of 20-40. Aristotle, who possessed a greater knowledge of natural history than any author of antiquity, remarks that the young of very old or very young animals are imperfect, and that the children also of very young or very old people are imperfect in mind and body. He asserts, too, that people should, for reasons that he alleges, marry at such ages that when the wife is in her 50th the husband should be near his 70th year, or that men should marry about the age of 37, women about the age of 18. In particular cases, as has been seen, to meet the infinite variety of social circumstances, greater disparities of age than these occur in Great Britain ; but the rule of Aristotle, if acted on universally, would work mischievously in various ways. Thirty-three women attain the age of 18 to every twenty-eight men who attain the age of 37 ; and the women of the age of 18 and upwards are to the men of 37 and upwards as 1402 to 804 ; so that a disproportionate number of the women would be unmarried. The proportion of widows would be increased, and fathers would less frequently live to see their children attain maturity. The object which Aristotle had in view is obtained by the re-marriage of widowers.

“The age of marriage cannot be directly fixed by laws ; but legislation, by prescribing the minimum age of marriage, and the age of majority, does exercise a considerable influence on good numbers of the people directly, and on all indirectly. It becomes the custom or the fashion not to marry below the age of majority. Thus in England about 9000 young persons of the age of 20 and under 21 married in the year 1851 ; while about 139,000 married in the four years after they were of age, as it is called, or in the years of age 21-25. The age of majority is 25 years in France ; and the age of 25 divided the minores from the majores in Roman law. This advanced age of majority, or of what becomes practically the lowest age of marriage, retards marriage indefinitely in many cases, and will probably be found, on investigation, to account, at least partially, for the comparatively small number of children to a marriage in France.

“By raising or depressing the age of majority, the legislature then has the power to exercise considerable control over the population.”  
—*Census of Great Britain*, 1851, vol. i. pp. xxxi. and xlvi.



## PART IX.

### THE DOCTRINE OF THE DURATION OF LABOUR.

THE progress of obstetrics is not characterised, as is that of some of the more exact sciences, by a secure and gradual advance with unassailable step, always conquering some part of the region of the unknown. Our science, seeking to enlarge the boundaries of what is certain and fixed, makes its conquests from the unknown in a field, wide indeed, and surrounding it on every side, composed, in its nearer parts, of doctrines more or less nearly approaching in stability to those admitted within the true boundaries of the science, but, in its more distant regions, of mere shadowy hypotheses, that have not yet acquired any roots, and of ephemeral conjectures, often the offspring of shallowness, of special pleadings, and of vanity.

It is my object to claim a place in the science of Midwifery for the doctrine of the Duration of Labour—a doctrine exceeded in importance by none within the limits of obstetrics, and having the most extensive bearings upon that invaluable art or practice of the accoucheur of which the science is the chief expositor. It and similar doctrines have been deprived of their real features and importance, and hid from general



appreciation by the violent and not always seemly struggles which have taken place upon them, and which have uniformly ended, like many battles with more sanguinary weapons, either in absence of real result, or in the more or less complete discomfiture of all the contending parties. But perhaps the medical philosophers of another age will have wisdom to regard, without pity or shame, these squabbles of our day as necessary episodes in the story of the progress of imperfect beings towards perfect truth—in the progress of human intellects towards real science.

The doctrine of the duration of labour has been the real centre of many discussions which have been invested with other names, derived from some therapeutical principle which has been supposed to receive confirmation or confutation from its bearings upon it. In these discussions the obstetric schools of Edinburgh and of Dublin have more than once been found on opposite sides, as if truth were indicated by different symbols in the two countries. A dispassionate inquirer, perusing these interesting discussions, will not fail to discover that, while each party had much truth as well as error in its arguments, each, with a blind zeal, attacked indiscriminately both truth and error in its opponents.

The chief practical questions which have been investigated in connection with the doctrine of the duration of labour are the artificial dilatation of the os uteri, certain other points in the management of protracted labour, turning as a substitute for cranio-



tomy, and the use of anæsthesia in midwifery. With these questions I shall not at present interfere ; only I may cite them occasionally to illustrate and facilitate the development of the great doctrine of the duration of labour, which is now my object. The names of Harvey, Denman, Osborn, Breen, Hamilton, Burns, Murphy, Collins, Simpson, and Busch, will always be honorably associated with the history of this doctrine. If in the sequel I do not frequently refer to all these writers, it is not because I lightly appreciate their labours, but because the subject appears to me to have now arrived at a stage at which it may with advantage be as far as possible dissociated from those various questions, which have been its parents, but would at present only injuriously encumber it. It is necessary to add that the two names preceding the last of those given, are involved in the latest dispute on this subject. In its various stages much talent was shown, and much truth elicited on both sides. With this last discussion I am best acquainted, and will naturally, therefore, refer to it more than to the views of the other authors distinguished in connection with the subject.

Into the questions we shall have to discuss the use of statistics has been introduced ; and it would be difficult to decide whether their application has tended more to elucidate or to confuse. It is evident that accurate statistics can never yield false results ; but false results are easily made to appear as if yielded by them. In other words, if a disputant resorts to statistics, without the most careful use of logic, he



easily flatters himself that they really supply the results he wishes from them. Against this fatal seduction into error many beacons have been erected, but they have not produced the safeguard desired by their sanguine authors. The present discussion, like many others in obstetrics, will afford clear examples of this abuse of a means of research which is among the most valuable on points where it is really available. If, in the preceding portions of this book, errors in statistical reasoning are to be found, they are not explained by the temptations of disputation, or by the allurements of a preconceived theory to be supported.



## CHAPTER I.

## THE DURATION OF LABOUR IN RELATION TO THE MORTALITY OF THE MOTHER IN PARTURITION AND CHILDBED.

IN this chapter we have to propound and prove two propositions.

*1st Proposition.*—The mortality of women in parturition and childbed increases with the increasing duration of labour (in an undetermined ratio).

*2d Proposition.*—The duration of labour is only an inconsiderable item among the many causes (single or combined) of the mortality of women in parturition and childbed.

These two propositions have hitherto been either confounded together, or made to conflict with one another. They really stand side by side, declaring separate truths, between which no collision can justly be made to arise.

*1st Proposition*—*The mortality of women in parturition and childbed increases with the increasing duration of labour (in an undetermined ratio).*

This proposition is one which easily gains credence,



when the obstetrician reflects on the abstract nature of it. It is one whose practical bearings are most remote and indefinite. But although this is the case, it enunciates a solid truth, and can never be with justice either neglected or depreciated. The proposition does not affirm anything whatever in regard to the influence of prolongation of labour upon the consequent maternal mortality; nor does it affirm anything whatever as to the dangerousness of the pains of labour. It affirms nothing in regard to any individual case. It merely asserts the general law, that as labours increase in duration, or become protracted, they are also accompanied or followed by a greater maternal mortality.

A proposition such as this scarcely requires proof. As labour becomes protracted, so does life; and we know that every hour of life added in adult age increases the mortality of mankind. But in the human female many dangers accompany the function of childbearing, and combine to raise, for the childbed month at least, the mortality of females very far above what can be accounted for by the mere general law applicable to all mankind. The dangers of childbearing are, for the most part, concentrated into the period of labour, or derive from it their origin. The longer the labour, there will be the more opportunities for such dangers to intervene; and hence it naturally follows, that the mortality of women in parturition and childbed increases with the increasing duration of labour.

But this proposition has been confirmed by nume-



rical investigations. I shall avail myself of Simpson's careful calculations,\* made from the data contained in Dr. Collins' admirable report of the Dublin Lying-in Hospital for a like purpose. Dr. Collins has in his report stated the duration of labour in 15,850 cases, of which 138 proved fatal. Table CXII. exhibits these cases, arranged so as to show that the maternal mortality increases as the duration of the process of labour is augmented. It requires no explanation or commentary.

TABLE CXII.

Duration of Labour.	Number of Deliveries.	Number of Deaths.	Proportion of Deaths.
Within 1 hour	3537	11	1 in 322
From 2 to 3 hours	6000	26	1 in 231
From 4 to 6 hours	3875	29	1 in 134
From 7 to 12 hours	1672	21	1 in 80
From 13 to 24 hours	502	19	1 in 26
From 25 to 36 hours	134	8	1 in 17
Above 36 hours	130	24	1 in 6

Such, then, is the statement and demonstration of this proposition.

It will be observed that the table of Dr. Collins' data gives us no information as to the special mortality of labours of extremely short duration, finished at various periods less than one hour. It is a very general opinion, and I believe a very correct one, that very rapid labours are, comparatively speaking, in-

\* *Provincial Med. and Surg. Journal*, 1848, p. 602.



jurious and dangerous. And more minute investigation, as to the relations of very brief labours to maternal mortality, will probably show that there is a limit, at some point within an hour, beneath which, if labours go on diminishing in brevity, they increase in mortality.

It must also be kept in mind that the peculiar case of primiparous women is included in the data. These have such peculiar conditions and dangers as must manifestly render their admixture with others prejudicial to the value of the data as demonstrating the proposition under consideration. It is desirable to have tables like that cited, composed of labours following pregnancies all of the same number.

It is not my purpose here to trace further than in a single author the history of this proposition. It has been stated, in terms almost identical with those I have used, by Professor Simpson, and confirmed by the table which I have adduced. To him, therefore, belongs the merit of formally enunciating it.\* This we admit, although it would be scarcely a stretch of literary justice to refuse him any credit whatever in connection with it ; for it will afterwards appear that he has so misunderstood and so used the principle, and the table on which alone he founds it, that his merit in the matter can be established only by separating the two or three sentences containing the bare principle and table from the mass of writing and argument in which he has enveloped them.

\* *Provincial Med. and Surg. Journal*, loc. cit. ; *Obstetric Works*, vol. i. p. 527.



We find this author first using the statistics of Table C'XII. to show that "the mortality accompanying labour is regulated principally by the previous length and degree of the patient's sufferings and struggles. In the Dublin Lying-in Hospital (he says), when under Dr. Collins' able care, out of all the women, 7050 in number, who were delivered within a period of two hours from the commencement of labour, twenty-two died, or one in every 320. In 452 of his cases, the labour was prolonged above twenty hours; and of these 452, forty-two died, or one in every eleven,—a difference enormous in its amount, and one surely calculated to force us all to think seriously and dispassionately of the effects of severe suffering upon the maternal constitution."\* Now, it is evident that these statistics afford no ground whatever for such reflections. No doubt, sufferings and struggles are important elements in the history of any labour or set of labours; but nothing in regard to the influence of sufferings and struggles upon the mortality of parturition can be wrested from the statistics adduced. These statistics support only the general proposition (the first) as to the relation of duration to mortality of labour. This relation is determined by a thousand circumstances, known and unknown, besides sufferings and struggles, in regard to the special baneful influence of which last it affords scarcely the slightest presumption.

\* *Monthly Journal of Medical Science*, October 1848; and *Obstetric Works*, vol. ii. p. 689.



When thus using Dr. Collins' data, Sir J. Y. Simpson was simultaneously engaged in his defence of anæsthesia in midwifery. In this cause, searching everywhere for arguments to convince Professor Meigs, he may be to a great extent excused, even when again falling into his former error in the use of these statistics. Addressing his transatlantic friend, and speaking of the pain of labour, he says, "It is safe in proportion to its shortness, and dangerous in proportion to its length. In the Dublin Hospital, the tables of which afford the only data on this point that I know to refer to, when the women were four hours in labour, more subsequently died than when their pain did not exceed two hours; of those that were eight hours in labour, more subsequently died than of those that were four hours ill; of those that were twelve hours in suffering, more died than of those that were eight: and so on, in a regular progression. The longer this supposed salutary and conservative manifestation of life-force (as Dr. Meigs terms it), the greater became the mortality. . . . etc."\* It is not to be wondered at that this argument did not convince Dr. Meigs, since it is as illogical in its use as it is wrong in its essence. What accoucheur could for a moment resist the argument, if true? It is not our object here to discuss the influence of painfulness, or sufferings and struggles, or, in short, of whatever anæsthesia could annul, upon the maternal mortality of labours; we shall only say, that all ac-

\* *Association Medical Journal*, July 1853, p. 582. *Obstetric Works*, vol. ii. p. 710.



coucheurs must recognise it as a great exaggeration, to imply that pain, etc., has any such immense influence as Dr. Meigs is asked to believe. Were it so, then anæsthesia should deprive parturition of its most formidable sources of mortality.

In defending his views with regard to turning in cases of deformed pelvis, we find the same author reverting to the same statistics of Dr. Collins for assistance. Here he supplies evidence against his own former use of these data, or *vice versâ*. For he now interprets them as affording "ample evidence that, contrary to the general opinion of the obstetric profession, the mere length of the labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process."\* But again, it will be evident, that these statistics afford no ground for attributing the maternal mortality to length or duration of labour as a cause, just as they afforded no ground for attributing the same mortality to the pain, etc., of the process. The proposition, that the increasing length of labour is accompanied by an increasing mortality, is a proposition at once true and proved by the statistics in question; while the proposition, that the "mere length of the labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process," is one, to say the least, very questionable, and one to which the statistics afford no countenance. It

\* *Provincial Med. and Surg. Journal*, Feb. 9, 1848, p. 58; and *Obstetric Works*, vol. i. p. 527.



is not necessary further to point out that, if the statistics so often referred to show that pain, etc., is the cause of the mortality, the same statistics cannot show that the mere duration is the cause of it; and if they prove either of these two points, they cannot be fairly extended so as to demonstrate our first proposition.

Dr. Collins justly objected to Dr. Simpson's uses of his data. The truth that was in them Collins rejected along with the error. A man of practical sagacity and immense experience, he at once repelled Dr. Simpson's erroneous conclusions, from the data in his *Practical Treatise*, in regard to the influence of pain and of length of labour upon maternal mortality. The inward testimony of his experience was so strong as to lead him instantly, and without analysing the statistical reasoning, to denounce these conclusions as visionary and extravagant. The truth of our first proposition he never grappled with. It had no apparent practical bearings; and therefore he refused to consider it.

Dr. Collins might have gone a little further. It would have been quite a legitimate use of Sir J. Y. Simpson's argument, as to the influence of length of labour upon the maternal mortality, to turn it against the whole practice of anæsthesia in midwifery. For it is a very general belief that anæsthetics, by diminishing the force of the uterine contractions, increase the duration of labour, at least in many cases. Hence it follows, if Dr. Simpson is right in regard to the baneful influence of mere length of labour, that anæsthesia



must tend to increase the maternal mortality. But as we have shown that the statistics do not demonstrate this baneful influence of mere length of labour, the opponents of anæsthesia are deprived of this otherwise strong argument provided for them by the greatest promoter of the practice.

Before advancing to the second proposition, I shall illustrate the errors fallen into with regard to the first by a reference to a subject long within the recognised domain of statistics.

TABLE CXIII.

Period of Life.	Proportion of Deaths.
At the age of 20 years	1 in every 141
„ „ 30 „	1 „ 99
„ „ 40 „	1 „ 77
„ „ 50 „	1 „ 74

This one hundred and thirteenth table may be assumed to be a correct statement of the mortality of mankind at different periods of life. An intelligent actuary will at once say, that it proves that the mortality of mankind increases with the increasing duration of life, just as he would recognise our former table as bearing direct testimony to the truth of our first proposition. But such an actuary will never say or admit that the adjoining table proves anything with regard to the sufferings and struggles, or pain, endured by mankind, or in regard to the effects of advancing life. It cannot be proved by our former table that the mortality accom-



panying labour is regulated principally by the previous length and degree of the patient's sufferings and struggles (nor is it true); so it cannot be proved by this table that the mortality of mankind is regulated principally by the previous length and degree of the individual's sufferings and struggles (nor is it true). It cannot be proved by our former table that the sufferings of labour are safe in proportion to their shortness, and dangerous in proportion to their length (nor is it true); so it cannot be proved by this table that the pains occurring during life are safe in proportion to their shortness, and dangerous in proportion to their length (nor is it true). It cannot be proved by our former table that, contrary to the general opinion of the obstetric profession, the mere length of labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process (nor is it true); so it cannot be proved by this table that, contrary to the general opinion of mankind, and of the medical profession, the mere length of life is a most serious and important element in reference to the degree of danger and fatality accompanying life (nor is it true).

*2d Proposition.—The duration of labour is only an inconsiderable part of the many causes (single or combined) of the mortality of women in parturition and the subsequent childbed.*

As we have, under our first proposition, cleared away many of the incumbrances of the whole subject, the treatment of this second will be much more brief.



There is no obstetrical doctrine more deeply impressed on all the valuable literature of our profession than this, that the mere duration of labour, considered in itself and apart from other causes of danger likely to spring up as the process becomes protracted, is of little importance, so far, at least, as recovery of the mother is concerned. This doctrine is embodied in the ever-recurring inculcation of patience, as the highest virtue of both mother and attendant, in many and various circumstances of distress during labour. Sometimes it is expressed in an apophthegm, "Meddlesome midwifery is bad;" at all times it is diligently instilled into the minds of young midwives and accoucheurs. Unlike our first proposition, a comparatively barren theorem, this is one of the best recognised and most valuable doctrines in obstetrics. It is, therefore, of the utmost consequence to defend and confirm it.

The proposition does not affirm that the mere duration of labour is of no importance,—quite the reverse. Far less does it affirm that the duration of labour, with the accompanying pain and struggles, is not a very considerable element in the history of every case. It says nothing in regard to the very important effects of the duration of labour after bad symptoms or dangerous complications have supervened. It asserts that the duration of labour is in itself (*per se*) only an inconsiderable part (probably a very inconsiderable

\* See Harvey's Works, Sydenham edition, p. 534, for his opinion of the influence of duration of labour.



part) of the many causes of the mortality of women from parturition and its consequences.

Perhaps the strongest evidence in favour of this proposition is the fact, that it is the ancient and generally received opinion of the profession.\* It rests upon what may be called the instincts of all experienced accoucheurs. In a science like medicine, where so little is capable of absolute demonstration, ancient traditions, especially if supported by the opinions of the great and wise, are among the most valuable and trustworthy guides of practice.

\* In attempting the defence of the opposite view, Sir J. Y. Simpson says—"I am fully aware that when I state my conviction, that the mere degree of duration and continuance of a labour is *per se* dangerous both to the mother and child, and very often fatal even in its influence, I venture to broach a doctrine which stands up alike against the opinion and the practice of some of the highest authorities in the obstetric profession.

"About half-a-century ago, when treating of the influence of the duration of labour in difficult and instrumental deliveries, Dr. Osborn observed—"I believe it is confirmed by general observation, that women recover at least as well after long, lingering, and laborious labours, the duration of which may have been extended to several days, as after the easiest, quickest, and most natural delivery.' In making this remark, Dr. Osborn stated, not his own opinion only, but, I believe, the general opinion of the accoucheurs of his time; and the same doctrine, little or not at all modified, still continues to be taught and acted upon, down to the present day, in the great English and Irish schools of midwifery, as the able and excellent writings of (for example) Professors Davis and Murphy, in London, and Drs. Collins and Beatty, in Dublin, etc., fully testify."—*Provincial Medical and Surgical Journal*, Feb. 9, 1848, p. 57.



But the proposition may be supported most satisfactorily, both by direct and indirect evidence. Were it true that, "contrary to the general opinion of the obstetric profession, the mere length of the labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process," then a well-established rule of philosophising must be declared to be at fault. It was a maxim of Newton's, that no more causes are to be admitted than are true and sufficient to explain the effects. Few indeed will ever be found to assert that any obstetric patient dies without a very evident, true, and sufficient cause. The causes of such deaths are very various no doubt; but the mere length of labour is, by Newton's maxim, excluded from the number, as the truth of its influence is in question, and it is not required to explain the phenomena.

Moreover, it is always true in nature that uniformity of cause insures uniformity of effect. This axiom also is at variance with the belief that mere duration of labour is an important cause of fatality in the process. For it is a common observation, that after long labours, even after the longest uncomplicated labours, there is often unusually rapid recovery. In the great mass of very long cases there is generally present some distinct and dangerous complication, which obscures the influence of the mere length of the labour, and destroys their value as arguments with regard to the effects of mere protraction. Again, in short and easy labours, where duration as a cause of fatality, supposed



by some to be supremely important, is absent, there is still a considerable mortality.

Dr. Collins has distinguished himself by his zealous defence of the doctrine embodied in our second proposition, maintaining, as he does, that the mortality from protraction of labour, apart from other causes, is comparatively small. His elaborate *Practical Treatise* contains no record of any patient dying from the mere length of the labour; and his experience, founded on his wide field of observation, leads him to consider mere protraction of labour an inconsiderable cause of maternal mortality. It would be difficult to adduce statistics, at least from Dr. Collins' work, to prove our second proposition. We have already shown how erroneously statistics framed from the data in his work have been used, and pushed forward as if proving that our second proposition is false. But some of Dr. Collins' data are almost as valuable as if they were positive proofs, from the light which they throw on the real causes of death in protracted cases.

To take one aspect of Dr. Collins' cases, as he has himself given it.\* Of 16,414 parturient women under his care in the Dublin Lying-in Hospital, forty-two died whose labours were longer than twenty hours. "Of the forty-two, three died of typhus fever; nine of puerperal fever; one of stricture of the intestine, with effusion into the thorax; three where the placenta was retained; two of convulsions; one of abdominal in-

\* *Provincial Medical and Surgical Journal*, Oct. 18, 1848, p. 573.



inflammation previous to labour ; nine of rupture of the uterus ; one of inflammation of the intestines, with pus in the uterine sinuses ; three of anomalous disease ; one of diffuse cellular inflammation ; six of inflammation, etc., subsequent to difficult labour ; one of ulceration and sloughing of the vagina ; one of disease of the lungs and hemorrhage ; and one of abdominal abscess." Here it is evident that we have a list of causes of death, apart from mere duration of labour, in all the cases where the length of the process exceeded twenty hours. No doubt the mere length of the labour may have been an aggravation in all these cases, but of this there is no evidence whatever in Dr. Collins' data, however arranged ; and we must accept the opinion of Dr. Collins, who took care of all the cases—an opinion sanctioned by previous general acceptance for ages, that protraction of labour was an inconsiderable part of the many causes of this maternal mortality in childbed.

The true bearing upon the great question before us, of the statement just quoted from Dr. Collins, has been altogether misconceived in some quarters. Dr. Collins' statement has been represented as "a list merely of such injuries and diseases as tedious labour does produce ;" and it is added, as if it were an apt illustration, that "long ago surgeons always used to argue, in regard to their lithotomy and other cases, that the deaths were from inflammation of the bladder, or inflammation of the intestines, or disease of the kidneys, or of the liver, or—anything, in fact, but the operation itself. Modern surgery (it is said) does not admit of such pathological



casuistry. Nor does modern midwifery.”\* It is scarcely worth while to stop to contradict the indiscreet reproach so easily cast upon old surgery and surgeons. Let us submit for a moment, and for argument’s sake, to consider it true—and only for a moment, as its irrelevancy will be easily made apparent. These old surgeons argued that their patients did not die of lithotomy, or of its consequences. Dr. Collins does not argue that his patients did not die of labour and its consequences; on the contrary he admits it. Dr. Collins argues, in opposition to Dr. Simpson, that the “mere length of labour was not a cause of death. To make a just use of the analogy above given, Dr. Simpson should have condemned the old surgeons for not considering the mere duration of the operation of lithotomy as a chief cause of the mortality of the operation. Dr. Simpson wishes us to condemn the old surgeons for not admitting inflammation of the bladder and intestines, etc., as causes of death in connection with lithotomy. In his zeal to prove the importance of mere duration of labour in reference to the fatality of the process, he censures Dr. Collins for admitting exactly analogous diseases as causes of death in connection with labour. Moreover, when Dr. Simpson speaks of “tedious” labour, he uses a well-known term, implying a great deal more than mere length of labour. When he says that tedious labour produces such diseases as Dr. Collins enumerates, then he and Dr. Collins are at one, and he

\* *Provincial Medical and Surgical Journal*, Nov. 1, 1848, p. 506.



had no right to address him as if committing a very great error. When he says that tedious labour produces these effects, he is not differing from, but agreeing with, the whole profession ; only, he is deserting the position which Dr. Collins attacked, and which he would fain appear still to hold. For his statement is, not that tedious labour leads to these causes of death—a true one ; but “that the mere degree of duration and continuance of labour is, *per se*, dangerous both to the mother and child, and very often fatal even in its influence ;”—a doctrine without foundation.

The element of mere duration of labour is, in fatal cases, so mixed up with other circumstances, that I despair of medical philosophers being ever able so to handle obstetric statistics as to make them yield anything like an approximation to a proper estimate of its baneful influence. In protracted cases, with no other evident dangerous complication, it is a common remark that the patients appear to make unusually rapid recoveries.

In tedious cases it is not the protraction which causes the complications and danger, but the complications which cause the protraction and danger, leaving the mere protraction as a negation destitute of any presiding influence.

Such is the statement of, and evidence for, our second proposition.

In the discussion between Dr. Collins and Dr. Simpson as to the influence of mere duration of labour



upon maternal mortality, we have seen that the latter, by his use of Table CXII., tried to prove that Dr. Collins was wrong in asserting that the mortality of mothers from protracted labour was strikingly small. Although Dr. Collins was not very happy in his statement of his views, and sometimes not to be justified in his arguments, yet there can be no doubt that the essence of the truth of our second proposition, as bearing on practice, was contained in his defence of his views.

Dr. Collins was personally engaged in watching and managing the great mass of cases reported in his valuable *Practical Treatise*. This circumstance will always give his views a peculiar force and value, even were his reputation as an author and observer not so high as it deservedly is. It was at least rash in any author, addressing Dr. Collins, to say—"Against the truth of your own recorded opinions I appeal to the truth of your own recorded facts. Against your own doctrines I appeal merely to your own data." Such are indeed very tame expressions compared with others that appeared in this controversy. And yet we think we have made it evident that Collins, in common with the general mass of the profession, was right in regard to the main question, and his opponent wrong. Any one who reads the controversy will find in it an admirable illustration of the fable of the two knights looking at opposite sides of the same shield. But, although to a careful perusal this becomes evident, it is only just to add that with Dr. Collins rested the



practical truth, fairly founded on experience, while some theoretical truth was fitfully maintained by his opponent, yet so as almost to be concealed by error.

Let us consider for a moment what such reasoning as Sir J. Y. Simpson adopts in this controversy would lead to. It appears to us that, if he had looked whither his arguments might lead, he would have himself been probably deterred from urging them. If mere length of labour be an important element in the causation of deaths from labour, then certainly patience is no virtue in an accoucheur. If mere length of labour be as he describes it, then meddling midwifery must, I fear, be declared good instead of bad. If mere length of labour be as important as he represents it, then any treatment which will accelerate delivery may be easily defended. If it be right to disregard all the real causes of danger and death in labours, as this author does, in order to make prominent the danger of protraction, with the ulterior view of supporting an artificial interference which accelerates the process, then a like reasoning may be used to support the most absurd and unjustifiable measures, and the art of midwifery will be at the mercy of any specious reasoner, however ill-founded his arguments may be.

It would be a waste of words to enter further on this discussion of the influence of the doctrine of the duration of labour. This doctrine has important relations to the mortality of children, in parturition, and



to other matters. It is enough here to point out that in connection with these questions the same errors in reasoning have been committed as have been made in tracing the bearing of the doctrine on the mortality of mothers.



## PART X.

### ON THE DURATION OF PREGNANCY.

IN many of the elaborate essays which have been written on the subject of the duration of pregnancy in women and in the inferior animals, it has appeared to me that an important source of error lies concealed.\* The exposition of it will, I trust, throw some light on this interesting subject ; and I am sure that, when it comes to be completely investigated, our notions as to the duration of pregnancy will be much more definite and satisfactory than they now are. I proceed to make a few remarks on this particular point, and then briefly to discuss the general question.

\* In the following passage Montgomery (*Signs of Pregnancy*, p. 503) evidently confuses insemination and conception. "It is, I think (says he), universally admitted that a woman may conceive on any day of the interval between one menstruation and another." Writing after the original publication of my remarks in the text, and probably referring to them, Montgomery admits the source of error pointed out, but illogically refuses it any place in his calculations. He says (p. 509)—"It has been suggested that pregnancy should be dated, not from the single fruitful intercourse, or insemination, which has produced it, but from the time when the ovum comes into contact with the semen masculinum, which union should be considered as constituting conception. Now, this may be true ; but supposing it so, how are we to make it available in practice?" (*Dr. Montgomery refuses to make it available even in reasoning.*) "Is not our attempting to do so more likely to engender confusion than to lead to satisfactory results?" . . . "If this view were adopted," he most erroneously believes and says, "we should have no means of calculating the period of gestation with anything like an approximation to accuracy in any case."



## CHAPTER I.

THE INTERVAL BETWEEN INSEMINATION AND  
CONCEPTION.

IN commencing it will be useful to define the meaning to be attached to some important terms frequently recurring in this discussion—viz. insemination, conception, and impregnation. By the word insemination is to be understood simply the injection of semen into the genital passages, the result of sexual conjunction. By conception is to be understood the more hidden and mysterious union of the semen and ovum; while the word impregnation implies both of these processes.

The confusion of the two former of these different processes is so general among obstetric writers that it is needless to quote authorities for the assertion. That they should always be kept distinct in studying this subject will, I hope, be made apparent. For, in fixing the commencement of pregnancy, it is necessary to date from the period of conception.\* Authors, in discussing this subject, have delighted to quote as

\* Joulin (*Traité Complet d'Accouch.* 1866, p. 449) points out this source of error. My remarks were first published in the *Edinburgh Medical Journal* for March 1854.



crucial examples those cases where the date of an only connection, or of connections within a short and limited time, could be satisfactorily decided. But it is evident that such a date only fixes the time of insemination, and not the time of the commencement of pregnancy; for a woman cannot be said to be pregnant whose body merely *contains* seminal matter. Pregnancy is a state of fertility, of breeding, which, as Leeuwenhoek long ago pointed out,\* cannot be said to commence until such time has elapsed as may intervene between insemination and the union of the ovum or ova and semen. This period of time, whatever may be its possible length, must be subtracted from all these supposed crucial cases of the duration of pregnancy. The interval described as the duration of pregnancy—that is, between successful insemination and parturition—must be considered as, in strict language, a false period; and it is so because it contains the period between insemination and conception, during which a woman is not pregnant. Of this interval, then, all such cases must be curtailed.

Very little has as yet been ascertained as to the

\* Hinc, hæc animalcula diutius in tuba sive matrice posse vivere, animo præsumebam meo, ac quoque nostræ mulieres non præcise eo die sive tempore, quo cum viro rem habuerunt, fecondas sive gravidas fieri; sed easdem post octo, aut decem, imo plures quidem dies, postquam coiverunt, gravidas posse fieri, quia post aliquot coitus dies ex multis saltem animalculis, unum animalculum eousque pervenire potest, ut punctum sive punctulum istud, animalculum fovendo aptum, attingat.—*Arcana Naturæ, etc.*, tom. ii. p. 150, edit. in 4to. Lugd. 1708.



possible length of this interval. It was my intention to have attempted to make it out in regard to some of the lower animals; but my inexperience in such investigations, and the pressure of other avocations, have hitherto deterred me from the pursuit of this object. There is, then, at present no resource in this question but to facts already known. Now, it has been ascertained by physiologists that for impregnation it is not necessary that the semen should be newly expelled by the male.\* Animals have been frequently impregnated, by Spallanzani and others, with semen, which has not only been kept for some time, but has even been variously altered, in certain properties at least, in experiments. And there seems to be no limit to the time during which the semen may be kept without losing its virtues, except the term of the life of the spermatozoa.

That this period is not insignificant, and cannot be passed over without risk of important error—in fact, that it may extend to many days or weeks—will appear

\* “On opening the body of a female mammal, one or more days after it has received the male, semen may be found not only in the body and horns of the uterus, but also in the oviducts, and on the surface of the ovary. The spermatozoa are in vigorous movement. These may retain their activity for a week or more in the female organs. And in many insects this period of time is much greater. Here the ova are only expelled long after copulation. The females, therefore, possess a special receptacle in which the moving spermatozoa are preserved until the ova finally reach them. In this receptacle their activity remains uninjured for many months.”—Valentin, *Text-Book of Physiol.* Eng. tr. p. 641.



from the following observations. We omit the facts in regard to animals so low in the scale as insects, in some of the females of which the semen is laid up in cavities where it retains its power for months. In regard to the dog, Leeuwenhoek\* pointed out that the spermatozoa might live for more than seven days preserved in a glass tube, and if such be the case in a rude experiment, it may be expected that they would retain vitality considerably longer in the passages of the bitch, where they have heat and moisture supplied under favourable circumstances. That they do live for some days in the genital passages has been proved by abundant observations, although the possible length of this period is not certain. The decision, indeed, of this point by microscopic observations would be a very difficult matter, as it would involve the almost impossible search for spermatozoa over every part of a long tract of mucous membrane. And this search would be necessary, with a view to deciding the question of the interval between insemination and conception, for we know by the experiments of Spallanzani that semen highly diluted, or, in other words, the smallest quantity of semen, is sufficient for impregnation.†

\* "Si enim animalcula plures quam septem integros dies in tuba vitrea vivere possint, quantum temporis illa in matrice, his animalculis recipiendis ac fovendis unice constituta, vivere quidem possent."—*Arcana Naturæ, etc.*, tom. ii. p. 150.

† These observations of Spallanzani have been considerably modified and corrected by the researches of Mr. Newport upon the quantity or number of spermatozoa required to fecundate an



The elaborate experiments of Haighton,\* long ago performed, show that in the rabbit conception generally does not take place till about fifty hours, or more than two days, after insemination. He found that division of the fallopian tube earlier than this time prevented conception, and that after waiting longer the conception was not prevented by the mutilation. It thus appeared that the conjunction of the ova and semen in the rabbit generally did not take place till more than two days after insemination. In the rabbit, then, there was found, in Haighton's experiments, this long interval between insemination and conception; and in some cases it is possibly much longer. In the rabbit the interval between insemination and parturition is ordinarily thirty days. The observations of Tessier upon 161 rabbits give five days as the extreme limit of the protraction of this term, a period of time which may be accounted for without any stretch of the space during which the semen may retain its fructifying power. And in this way it may have happened that the real period of gestation—that is, from conception to parturition—may not have been at all protracted in these cases. The cases also in which the period was less than thirty days may be explained by supposing the ova to have been further matured or even advanced into the uterine horns before impregnation took place, so that conception may have happened very soon after ovum in the frog, etc. See his paper in the *London Phil. Trans.* for 1853, part ii.

\* *Philosophical Transactions*, 1797.



insemination. And in Tessier's observations it is remarkable that in none of the rabbits did labour anticipate the usual time more than two days, the period which Haighton's experiments seem to show to be the usual interval between insemination and conception in that animal. In the present state of our knowledge, however, these explanations cannot be considered as absolutely established.

Experiments of Cruikshank upon the rabbit and doe, experiments of Wharton Jones, Martin Barry, and others, might be adduced as throwing light on this point.

For reasons which do not require to be stated, there is great deficiency of evidence in regard to the analogous subject in the human female. But there is every reason to believe that the circumstances of conception in her closely resemble those in the higher animals. It has of late years been shown that, in woman, at every menstrual period an ovum is matured and expelled from its graafian vesicle, and that she is liable to conceive during its progress along the fallopian tube. How long after its maturation the ovum can retain its vitality and susceptibility to the seminal influence is not known, but probably the time is short. Nevertheless, cases might be easily adduced from the works of eminent obstetricians to prove that a single insemination at any period of the interval between two menstrual periods may result in the fertilisation of the female. Of such cases those only are important from our present point of view where conception has



resulted from insemination shortly before the return of a period. They admit of explanation in three different ways.\* Either the ovum has remained up till this time entire and susceptible of being influenced by the semen; a supposition which is very improbable as regards the ovum,† and is at variance with what we know of the history of the decidua or nidus prepared for the egg's further development. Or the excitement of connection may have hastened the maturation and rupture of a graafian vesicle; a view which is in itself improbable and inconsistent with what we know to result from similar circumstances in the lower animals. But it may also happen, and I believe it does not unfrequently happen, that the seminal animalcules remain in the passages till the ovum is prepared and discharged from its vesicle. An objection at once appears to this explanation—namely, that these spermatozoa would be removed by the menstruation con-

\* As a good example we may refer to a case of Dr. Montgomery's (*Signs, etc., of Pregnancy*, p. 258). The last menstruation was on the 18th October. Insemination took place on the 10th November; parturition on the 17th August. The interval between insemination and parturition was thus 280 days; between last menstruation and parturition it was about three weeks more.

† "The passage of the ovum from the ovary to the uterus occupies, M. Bischoff says, three days in the rabbit, and four or five days in ruminants, and therefore probably eight or ten days in the human female. M. Bischoff believes that the ovum escapes from the graafian follicle at the time when the menstrual discharge is about to cease, and he is of opinion that in order to be fecundated it must be acted on by the semen while it is in the fallopian tube." —Baly and Kirkes' *Suppl. to the 2d vol. of Müller's Physiol.* p. 58.



temporaneous with the discharge of the ovum. When menstruation does supervene on a single recent coitus, this will probably happen, unless the semen have permeated the fallopian tubes, and thus advanced beyond the scope of the menstrual flux. But the study of such

\* Several authors have stated their belief that the mucous membrane of the tubes yields a contribution to the bloody menstrual flow. I have seen the mucus of the tubes tinged with blood in the autopsy of a woman dying during menstruation, and in some other cases. Cases of occlusion at the uterine extremities of the tubes, in which they have been found distended with bloody fluid (see Bernutz et Goupil, *Maladies des Femmes*, vol. i.), are upon record. Numerous other instances of repletion of the tubes with blood or bloody fluid are to be found, but in these last there is generally no good evidence that the blood was derived from the tubes themselves. It is, however, sufficiently well demonstrated that in some cases blood is excreted from the mucous membrane of the tubes in small quantity. It may be regarded, I think, as nearly certain that, in natural menstruation and in menorrhagia, blood is not excreted from the tubes in considerable quantity. The statement by Tuckwell (see his thesis *On Effusions of Blood in the neighbourhood of the Uterus*, p. 7) of his opinion to an opposite effect, is unsupported by any adduced evidence. Were the uterine ends of the tubes as they are generally described, and did the mucous membrane of the tubes supply, as Dr. Tuckwell believes, no inconsiderable part of the blood that escapes in what is called an attack of menorrhagia, then great hæmatoceles would be frequent to a very much greater degree than any one at present supposes them to be. Further, the anatomical demonstrations of Rouget are hostile to the tubal source of menstrual blood. I have already said that I have seen the mucus of the tubes tinged with blood : I have also seen the mucous membrane of the uterine extremities of the tubes detached—hornlike—with the proper decidua uterina in abortion ; the hornlike tubal projections from the uterine decidua measuring three lines in length.



cases as recorded by various authors \* reveals this interesting fact, that under such circumstances menstruation often does not take place at all, or only very scantily ; the uterine system, as it were, anticipating the conception, and preventing the failure which might result from a free discharge of blood. It is evident that such cases occurring in married women would be very liable to be considered cases of gestation protracted a month.

Some actual observations on women, bearing on these points, have recently been published by Dr. Marion Sims.† They demand quotation. “It would be important (says he) to determine how long spermatozoa can live in the matrix. On this point we need more extended experiments, for I do not think that their duration of life has yet been fully established. Dr. S. R. Percy (*American Medical Times*, March 9, 1861), of New York, reports a case in which he found ‘living spermatozoa, and many dead ones,’ issuing from the os uteri eight and a half days after the last sexual

This detachment from the tubes appears to be analogous to that occasionally observed from the cervix uteri : in both situations some little blood may, no doubt, be excreted in natural menstruation, but neither does afford the hæmorrhage in menstruation or menorrhagia.

\* Mauriceau (*Maladies des Femmes Grosses*, obs. 676) mentions a case interesting in this point of view, in which a woman was impregnated during the flow of menses.

† *Clinical Notes on Uterine Surgery*, p. 384. For a similar statement see Joulin, *Traité Complet d'Accouch.* p. 449.



connection. During this time the husband of the patient had been from home.

“ I have examined the semen many times with the view of determining this point, and think I can safely say that spermatozoa never live more than twelve hours in the vaginal mucus. But in the mucus of the cervix they live much longer. At the end of twelve hours, while all are dead in the vagina, there are but few dead ones to be found in the cervix. When the cervical mucus is examined from thirty-seven to forty hours after coition, we shall ordinarily find as many spermatozoa dead as alive. But my observations on this point could not, under the nature of things, be accepted as the rule, for they were all made upon those who were, or had been, the subjects of uterine disease in some form or other.

“ Here is the report of an observation made upon a patient who is perfectly reliable : ‘ Sexual intercourse at eleven P.M. on Saturday. A microscopic examination of the secretions was made on Monday at three P.M., just forty hours afterwards. The vaginal mucus contained a few dead spermatozoa—none alive ; the cervical mucus contained great numbers very active—a few dead.’

“ The above is copied from notes made at the time. I saw no reason why many of these active spermatozoa should not have lived for a still longer time. Many of them lived six hours after their removal. This was in July.”



I refer my readers to the same author\* for some interesting cases of impregnation shortly before an expected menstruation which did not make its appearance.

\* *Clinical Notes on Uterine Surgery*, p. 381.



## CHAPTER II.

THE INTERVAL BETWEEN INSEMINATION AND  
PARTURITION.

THIS is a period of the greatest importance in a medico-legal point of view. It is discussed by obstetric authors as the period of gestation, or as the term of the duration of pregnancy. We have already shown that the present state of our knowledge requires us to make a distinction between the date of insemination and that of conception, and it strongly appears to us that the full comprehension of the bearings of this distinction will go far to equalise the discordant views as to the term of pregnancy in the human female, and to account for many of the so-called cases of prolonged gestation. But with our present ignorance of the possible interval between insemination and conception the exact attainment of this result is impracticable.

In attempting to settle this point, authors have resorted to numerous sources of evidence, the fallacy of which they themselves well knew. For instance, we find Dr. Montgomery in his classical essay on the period of human gestation, and many other authors,



quoting examples based upon the evidence of peculiar sensations felt at the moment of conception, on the last appearance of the menses, and on the time of quickening—phenomena which, however important in aiding the accoucheur to make a good guess of the day of confinement in single cases, can never be for a moment relied upon in deciding such an exact question as that before us. An excellent story, illustrating the fallaciousness of such evidence, is related by Dr. Reid, of an expert midwife who, when examined in the celebrated Gardner peerage case, “deposed that she had once gone ten months with child, that she was always right in her calculations, that she always fainted away at quickening, etc., so that she could not be deceived.”\* Some time after the trial she applied to Dr. Reid, convinced on such grounds that she was seven months pregnant. But on examination there was found no pregnancy at all.

No reliance can be placed but upon accurately-ascertained dates of parturition and of fruitful connection. In regard to the latter of these dates no confidence can be placed in the statements of women living habitually with males, however truthful they may be, or whatever additional evidences they adduce. We are therefore reduced to a limited class of observations—namely, those where the pregnancy resulted from a single coitus, including those where this never took place but on a single day, and those where it was removed on both sides from other similar occasions by

\* *Lancet*, vol. ii. p. 78, 1850.



months, or such other period as would render it absurd to refer the parturition of a fully-developed foetus to them. With those dating from a single day we have included some dating from one of two days, but in such cases our calculations commence from the coitus of the first day only. These statistics (for the details of which we refer to the note)\* contain 46 cases, which yield the period of 275 days as the average interval

\* Raciborski (*De la Puberté*, etc., p. 460, etc.) relates five cases which come within this category. The intervals were 275, 270, 268, 273, and 274 days respectively. Montgomery in his work on the *Signs, etc., of Pregnancy*, quotes or relates seven cases. The intervals were 281, 280, 287, 289, 288, 284, and 291 days respectively. These cases differ manifestly from those of Raciborski, but this is accounted for by observing that, like some of those yet to be quoted, they are selected by Montgomery as proofs of the prolongation of pregnancy in some cases. Rigby, in his *System of Midwifery* (p. 84), mentions three cases. The intervals were 260, 264, and 276 days respectively. Reid, in his elaborate essay on the "Duration of Pregnancy" (*Lancet*, vol. ii. 1850), notices twenty-five cases. The intervals were 276, 274, 274, 275, 273, 271, 274, 274, 278, 263, 280, 264, 274, 276, 274, 276, 280, 266, 265, 266, 272, 275, 271, 287, and 293 days respectively. Besides many of those already mentioned, he adds five cases from the *American Journal of Medical Sciences*, which were 270, 272, 276, 284, 272 days respectively, and Mr. Skey's case of 293 days. All the above are carefully-selected cases, where the date of coitus taking place only during a single day, and the date of parturition, were accurately ascertained. They are in all 46 cases. The average interval is 275 days. More than two-thirds of the cases have an interval of 276 days or less. Other collections of cases have been made with great care. I may especially refer to those of Veit (*Verhandlungen der Gesellschaft für Geburtshülfe in Berlin*, hft. 7, 1853, S. 122) and Hecker (*Klinik der Geburtskunde*, S. 35). These make this interval about 273 and 277 days.



between insemination and parturition.\* While 275 days was the average interval, it may be remarked that the largest number of cases at any particular day was 7 at the 274th day.

\* In France, 270 days is the ordinarily-accepted duration of pregnancy. See the works of Jacquemier, Velpeau, etc.



## CHAPTER III.

THE INTERVAL BETWEEN THE LAST MENSTRUATION  
AND PARTURITION.

THIS is a period which, for obvious reasons, can be much more easily and frequently ascertained than that last under discussion. It is one the knowledge of which is of the greatest practical importance in the every-day life of the married female, and of the obstetric practitioner, seeing that by aid of it he attempts to predict the date of the expected confinement. In the vast majority of cases it is the only fixed point from which the calculation can be made, and hence the necessity of accurately ascertaining it, if possible.

Authors have frequently neglected the discussion of this important period, the only one available in most cases of pregnancy. They generally decide the term of pregnancy theoretically, and upon insufficient grounds, and direct that, in calculating for the day of confinement, this term should be told off from some day after the last menses, which day they conceive to be that on which conception most frequently or most probably takes place. For instance, Montgomery



states, upon the evidence of a very few cases only, that the natural period of human gestation is 280 days, and in calculating the date of parturition, recommends this to be added to any day within a week after the last menstruation. He thus includes between the last menses and the date of parturition a period varying from 281 to 287 days—a period which, we shall show, considerably overpasses the mark. Other authors and teachers, considering that a woman is equally liable to conceive on any day between two menstrual periods, direct that the middle day of that interval be taken, and the supposed period of gestation, 280 days, added thereto—thus including the exaggerated space of 290 to 295 days between the last menstruation and parturition.

The exact determination of this interval, as of that last under discussion, can be obtained only by a reference to actual observations. Modern researches have shown that it is at the menstrual period that the ovum quits its graafian vesicle, and traverses the fallopian tube on its way to the uterus. It is in the course of this passage that it encounters the semen, and conception results. This passage occupies about three days in the rabbit, and in M. Bischoff's opinion it occupies eight or ten days in woman. During all this time, then, the woman will be liable to conceive. It will, therefore, be expected that the interval of which we are at present speaking will be some days, at least, longer than the last.

The statistical calculations on this subject (for de-



tails see foot-note),\* give on an average 278 days as the interval between the last menstruation and parturition—a period less even than the 280 days which we have generally been taught in this country to be the interval between impregnation and parturition, or the duration of pregnancy.†

The largest numbers of cases on particular days conglomerate about the 280th. Among Dr. Reid's 500 instances, 283 were within the 280 days, and 217 beyond it. So far is it, then, from 280 days being the ordinary duration of pregnancy, that a woman generally does not go more than 278 days after the last menstruation is over. This period exceeds the average interval between insemination and parturition by three

\* The valuable statistics from which these results have been derived, by a tedious calculation, are published by Drs. Merriman and Reid. The observations of the former were originally published in the 13th volume of the *Medico-Chirurgical Transactions*, and subsequently extended in the edition in 1838 of his work on *Difficult Parturition*. The observations of Dr. Reid are to be found in the 2d volume of the *Lancet* for 1850. In Simpson's paper on the "Duration of Human Pregnancy," these and other allied statistics will be found carefully elaborated. See *Monthly Journal* for July 1853. In a statistic which I have made of the cases having sufficient details, recorded in the books of the Royal Maternity Hospital, a result comes out similar to that derived from the far more extensive records above mentioned.

† "The common term of pregnancy (says Smellie) is limited to nine solar months, reckoning from the last discharge of the catamenia."—*Treatise on Midwifery*, fifth edition, vol. i. p. 127.



days ; and we may argue from this, with some little probability, that conception takes place generally a few days after menstruation is finished—a view which is confirmed by numerous other physiological observations. Sexual connection in the days immediately following menstruation is generally believed to be especially likely to produce impregnation.



## CHAPTER IV.

## THE PREDICTION OF THE DAY OF CONFINEMENT.

THIS is one of the functions ascribed to the accoucheur ; and apart from the comfort and convenience which the mother experiences from the foreknowledge of it, she often makes its failure or success a test of the more subtle acquirements of the physician. The foregoing statistics, however, will always justify the physician in never giving a decided prognosis of the day of confinement ; and if he have been guarded and careful, will afford him an asylum, showing, as they do, that with the most certain knowledge of the termination of the last menstruation, or even of the date of a single coitus, no safe prediction can be made unless within limits so extended as to deprive it of much of its value. At the same time there is no doubt it will always be desirable to know the most probable day of confinement, and this can generally be settled with some exactness.

If the date of a single connection is ascertained, which is, of course, very rarely the case, then the process of deciding the probable day of confinement simply consists in telling off 275 days (the average interval



between insemination and parturition) from that date. Now, any nine consecutive calendar months include 275 days, if February is not in the number. If February is in the number, the nine calendar months include only 273 days (leap-years excepted), and the correction necessary is apparent. The whole process of calculation, then, consists in attaching the number of the day of connection to the name of the ninth succeeding month, and adding two additional days if February is included in the interval.\*

In the vast majority of cases the day of confinement is predicted from the date of the termination of the last menstrual period. In many cases the calculation can be aided and corrected by comparison with former pregnancies in the same female. But when this source of information is wanting, the nearest approach to truth will be made by adding to the day of the disappearance of the menses 278 days (the average interval between the end of menstruation and parturition). The prediction will, of course, prove erroneous in a great number, nay, in the majority of cases, but it forms the nearest approximation which the mother can obtain to guide her. If a woman, then, knows the last day of her last period, she has only to tell the same day for the ninth month following (most mothers do so on their fingers, which thus form an admirable periodoscope), and add

\* Nine months do not always contain 275 or 273 days. Dating from December and July, nine months contain 274 days, and from May 276. The statements in the text, although sufficiently correct for general use, require this correction to be exact.



three days, or, if February is in the interval, five days. She thus has the most likely day of her confinement; or, perhaps better, she has the middle day of the week in which she will probably be laid up.

I have already casually shown how this varies from the calculations ordinarily recommended by most British authors and teachers. It would be tedious to enter further on this subject. I may merely remark that a more correct plan prevails on the continent. And from some inquiries and observations I have made in Scotland and England, I find that, popularly, a more correct calculation is extensively in use than that recommended in the schools. For instance, in Edinburgh, and some parts of Scotland, it is common to find women calculate in this way. They find the last day of being menstruated, and they hold that the same day nine months after will be the day of confinement. The celebrated Harvey's opinion on this subject was also very correct. His remarks tally with Dr. Tyler Smith's ingenious views on the question, and are deserving of quotation:—"Unquestionably," says he, "the ordinary term of utero-gestation is that which we believe was kept in the womb of his mother by our Saviour Christ, of men the most perfect; counting, viz., from the festival of the Annunciation, in the month of March, to the day of the blessed Nativity, which we celebrate in December. Prudent matrons, calculating after this rule, as long as they note the day of the month in which the catamenia usually appear, are rarely out of their reckoning: but after



ten lunar months have elapsed, fall in labour, and reap the fruit of their womb the very day on which the catamenia would have appeared had impregnation not taken place.”\*

\* *Harvey's Works.* Willis's trans. p. 529.



## CHAPTER V.

## PROTRACTION OF THE PERIOD OF PREGNANCY.

PROTRACTION beyond the common or natural term is a phenomenon which most obstetricians are now willing to admit. Protraction beyond the average term is a matter of course: it is only extraordinary lengthening of pregnancy, the child being alive, that is here spoken of. Although believing in its possibility, I am at the same time convinced that it is not so frequent an occurrence as late writers on this subject seem to think, and that many of the cases of this kind which are recorded have not sufficient evidence to support them. They are mostly based upon such signs as the disappearance of the menses, the sympathetic phenomena of pregnancy, and a physical examination of the uterus; all of which, it is needless to say, are abundantly liable to create misapprehensions and fallacious reasonings, and, singly or combined, can justify no absolute conclusion from them. The kind of evidence desiderated is that based on pregnancies produced by a single coitus: but, as protracted pregnancies are rare, such evidence must be very difficult of attainment. One great reason for



discrediting the evidence of most of the cases recorded by authors is, that we hear nothing of great development of the uterus, or of large size of the child or of the placenta, in such cases—results which, to say the least, might be expected. On the contrary, we find authors stating that in these so-called cases of protracted pregnancy the child is no bigger than usual, or is even smaller than ordinary. “Although in some of the cases of protracted gestation,” says Dr. Montgomery,\* “the child was of enormous size, it by no means follows that it should be so in all such instances; and, in point of fact, we find it expressly mentioned in some of them that the child was smaller than usual, as happened in one of Dr. Hamilton’s cases; and Foderé says, that in three instances in which gestation was evidently prolonged the children were under-sized and ill-thriven; while, on the other hand, the largest children are often produced where no extension of the term could have taken place.” Dr. Burns also says† that “some causes which we cannot explain nor discover have the power of retarding the process (of gestation), the woman carrying the child longer than nine months; and the child when born being not larger than the average size.” In further corroboration of these views the valuable observations on cows by Tessier have been cited as showing that there was no marked coincidence of increase of size and weight of the

\* *Signs and Symptoms of Pregnancy*, p. 282.

† *Principles of Midwifery*, p. 199.



foetus with protraction of gestation. But this reasoning from analogy between the cow and woman appears to be very much overstretched, and there are evident reasons for expecting, *à priori*, that the period of gestation in woman should be limited on the side of protraction more than in the lower animals. Of these the strongest is based on a consideration of the adaptation of the well-developed nine-month foetal head to the maternal passages, and the evils that are so well known to result from even slight disproportion between them. And, unless it be supposed that pregnancy is protracted for the special behoof of small and ill-developed children, it must be admitted that an extraordinary development of the foetus is to be looked for in such cases. The acknowledged absence, then, of this extraordinary intra-uterine development is, for me, a strong evidence against the reality of many so-called cases of prolongation. On the other hand, the presence of this sign in addition to others is, in my opinion, powerfully corroborative of the supposed protraction in any instance. In illustration of this I may state that the best example I have met with of probable protraction occurred in a female who had borne several children, and who had previously always been correct in the calculation of the period of confinement from the cessation of menstruation. On the occasion in question she passed her calculated time four weeks, and before confinement expressed her conviction all the more strongly in consequence of my incredulity, that



she had passed her time a month. The labour was more tedious than usual in consequence of the great size of the foetal head. The child proved of very large size and advanced development. It weighed 10 lbs. 4 oz. The placenta was 2 lbs. in weight. Other cases similar to the above have come under my observation, some have been communicated to me by professional friends, and some are to be found recorded. Among these last I may cite the observation of Smellie, in which he says it was reasonable to suppose that the patient actually exceeded the usual term of gestation by four or five weeks at least. "Her labour was very tedious, though the pelvis was of a large size; but the child was very lusty, and the head squeezed into a longitudinal form. It was," he adds, "the largest child I ever brought into the world."

In these cases the ordinary sources of evidence were confirmed by the evidently exaggerated development of the ova, the results of these protracted pregnancies. I have lately had under my care two cases in which gestation was, not without some reason, supposed to be prolonged, but which I reject from this category, because, although the ladies were in good health at the time of falling in the family way, yet the infants born were not at all larger than their former children. The ladies were sisters, and in each of them their calculation and mine was passed by nearly a month. The data founded upon were the cessation of menstruation and the occurrence of morning sickness. In both cases the respective nurses were residing with



them for about a month before the supervention of labour.

Such cases as those of the two sisters just mentioned, and numerous other so-called cases of protraction, are easily explained by supposing simply that the menstrual flux which should have occurred about the probable time of the fruitful intercourse was suppressed ; or, in other words, that the decidua prepared for the ovum destined to be impregnated did not as usual throw off the bloody fluid. In these cases we must suppose either that the suppression for this one period arose from some ordinary constitutional cause, or, what is more likely, that the fruitful intercourse occurring shortly before the ordinary menstrual period anticipated and prevented it. This phenomenon we believe to be not very rare, and to be sufficient to explain away many cases of protracted gestation. In further illustration of this circumstance, we must be satisfied with referring to those cases of pregnancy after a single coitus taking place shortly before menstruation, the coitus producing, firstly, the partial or complete suppression of the menses at the approaching period, and secondly, the fertilisation of the ovum discharged in coincidence with the suppressed period. Some careful observations of this sort are recorded by Raciborski, Montgomery, Marion Sims, and others.

Till the question is brought near to a settlement by the accumulation of instances of protraction, in which only a single coitus could have fertilised the ovum, we must rely on individual opinions and indi-



vidual instances. I have already expressed my opinion, and may add that I do not know of an individual instance of certainly protracted gestation in which the production was small and light in weight. Before authors, as Hamilton, Foderé, and Montgomery, can justly ask the assent of the profession to their opinion, they must give the evidences or proof of protraction in the individual examples relied on. It is not sufficient that they express their opinion that they were evidently prolonged, in a case where their opinion regarding the resulting small children is contrary to what might naturally be expected. I have already cited cases, equally well ascertained, opposing the view of Hamilton, Foderé, and Montgomery. The opinion of Tessier, and his observations on cows and mares can, only by analogy, be brought to bear on the physiology of woman. But, their value being admitted, it is to be remembered that his facts number only eleven, and that no average weight of calves and foals is adduced for comparison with the special instances. Tessier's examples, besides, are not all quite pertinent, for they include two classes of cases—namely, when a small foetus was produced after a prolonged gestation, and when a large foetus was produced after a short gestation.\*

As such opinions and examples have interest in

\* *Mem. de l'Acad. Roy. des Sc.*, 1817, p. 18. In his work, *Von dem Mangel, etc., der Gebärmutter*, p. 308, I find a passage indicating that Kussmaul admits the possibility of a small foetus resulting from a prolonged pregnancy.



the present state of the question, I shall add some more. And first I shall refer to a paper by Mr. Annan,\* who in his turn cites an example of a large child following a long pregnancy, recorded by Dr. Collins of Liverpool. Mr. Annan gives three good instances, and expresses himself decidedly in favour of the view which I defend. Discussing difficult labours, Dr. Tyler Smith remarks, "The largest children I have met with in practice have been in cases where, from pendulous abdomen, the foetus had been retained beyond the full term."† From Joulin‡ I make the following quotation:—"Manvie, vétérinaire à Epe, observa une vache qui porta près de 16 mois. Le volume du veau, qui pesait 61 kilog., rendit le part impossible; on abattit la bête. En 1831, on reçut à l'Ecole Vétérinaire d'Utrecht, une vache qui porta 15 mois moins 2 jours. Numan constate une gestation de 11 mois  $\frac{1}{2}$  chez une vache. Le veau qu'on fut obligé d'extraire par morceaux pesait 80 kilog. Enfin, Gronier à observé une vache qui porta 12 mois. Le petit, extrait vivant, avait acquis le volume d'un veau âgé de deux mois. Ces faits peuvent paraître étranges, insolites; mais, je le répète, on n'a véritablement aucune raison pour en nier la réalité. Louis, et les autres adversaires des naissances tardives, invoquaient à l'appui de leur opinion, l'ordre immuable des phénomènes de la nature. Ce sont là des phrases

\* *Edinburgh Medical Journal*, 1857, p. 712.

† *Manual of Obstetrics*, p. 449.

‡ *Traité Complet d'Accouchements*, p. 456.



vides de sens ; car on constate à chaque instant que la nature s'écarte de cet ordre prétendu immuable. On a dit que la cause des grossesses prolongées, dépendait du développement imparfait du fœtus qui séjournerait dans l'organisme maternel jusqu'à ce qu'il ait acquis toutes ses aptitudes à la vie extrautérine. Il est possible que cette opinion soit parfois justifiée, mais l'obstétrique comparée nous prouve qu'il est loin d'en être toujours ainsi. Et je crois que dans un cas de grossesse prolongée bien avérée, il ne faudrait pas s'exposer à subir les conséquences d'un développement exagéré du fœtus, surtout chez une femme dont le bassin aurait des proportions médiocres. Dans ces circonstances on pourrait poser avec Silbert (d'Aix), la question de l'accouchement provoqué. C'est un point pratique qui mérite l'attention."

There is a large field here open for research, not only as to the connection of protracted pregnancy with development of the child, but on the connection of age of the mother and of the number of the pregnancy with the duration of the process and the development of the resulting progeny, topics on which I have made some remarks in an earlier part of this volume (see page 63).

The evidence of highest value which we possess in regard to the subject of the extraordinary prolongation of pregnancy is founded upon cases where pregnancy resulted from a single connection. The results of these cases go far to establish the well-founded opinion of Dr. Montgomery, that the cases most de-



serving of confidence are those in which the usual term was not exceeded by more than three or four weeks. But the cases referred to give us the interval between insemination and parturition, a period which I have elsewhere remarked requires a correction, which physiology has not yet enabled us to decide, for the possible interval between insemination and conception. In a practical and medico-legal point of view, however, the interval obtained is of great importance. In the collection of cases of this kind (see p. 333), the longest duration found is in one case where the period was 293 days. The other cases of protraction will be observed by a reference to the table.

The theory of the duration of pregnancy is still unknown. Some authors, believing that labour comes on at the tenth menstrual period, explain the protraction by the female's having a longer menstrual interval than usual, ten of which will make up a period exceeding the usual term of pregnancy. Others have supposed that from some cause a female might miss the usual period and go on to what would have been the next menstrual period, had she not been impregnated. Others have connected it with tardy development of the foetus, with the influence of depressing emotions, etc. But all these are as yet mere hypotheses.



## CHAPTER VI.

## DR. MONTGOMERY'S OPINIONS.

IN the *British and Foreign Medico-Chirurgical Review* for July 1854 there appeared an abstract of a paper, by Dr. Gustav Veit, professor of midwifery at Rostock, on the duration of pregnancy and other subjects. I have had no opportunity of seeing more of this essay than is contained in the midwifery report of the above journal. Dr. Veit has tabulated forty-five cases from Reid, Montgomery, Girdwood, Rigby, Lockwood, Lee, Desormeaux, Dewees, Beatty, Skey, M'Ilwain, Ashwell, Cederschjöld, and others, in which the date of impregnation appeared to be fixed by a single coitus. From this table it is found that the average interval between insemination and parturition (commonly called the duration of pregnancy) is 276·93 days.

Dr. Veit also collected a mass of observations in regard to the interval between the end of menstruation and parturition. From this collection he ascertained that the average extent of this period is 278·5 days.

These and like conclusions, though widely published, had been subjected to nothing that could be called adverse criticism till the recent republication of



Dr. Montgomery's great work on the *Signs and Symptoms of Pregnancy*. This contains an essay on the period of human gestation, in which it is assumed, we shall see on what authority, that the natural period of gestation is forty weeks, or 280 days; and in which the following statement of the immortal Harvey is pronounced to be erroneous. We quote from Montgomery.

“The words of this illustrious man are these—‘*Unquestionably the ordinary term of utero-gestation is that which we believe was kept, in the womb of his mother, by our Saviour Christ, of men the most perfect; counting, viz., from the festival of the Annunciation, in the month of March, to the day of the blessed Nativity, which we celebrate in December.*’ This is a period of 275 days only: he then goes on to state, what does not appear to have any very obvious connection with the fact referred to, but is indeed rather at variance with it.”

“‘*Prudent matrons,*’ he says, ‘*calculating after this rule, as long as they note the day of the month in which the catamenia usually appear, are rarely out of their reckoning; but after ten lunar months have elapsed, fall into labour, and reap the fruit of their womb the very day on which the catamenia would have appeared had impregnation not taken place.*’”

I defend the opinion of Harvey on the ground of the data afforded by Dr. Montgomery, and shall show that his assumption of 280 days as the natural period of human gestation, is, to say the least, unfounded.



The period generally recognised under this name, and discussed by Dr. Montgomery, does not measure the real duration which extends from conception to parturition, but that other period extending from fruitful connection to parturition. It is this latter of which we now discourse.

Dr. Montgomery describes the natural period of human gestation as 280 days. Now, there is no such thing known to obstetricians to exist in nature as a natural period of pregnancy, measuring a certain number of days. This may be considered absolutely demonstrated. The interval between fruitful coitus and parturition is known to us only as a variable period, of uncertain length, not merely in different individuals, but even in the same individual on different occasions. So far is Dr. Montgomery from having any authority for fixing 280 days as the natural period, that, in his own laborious collection of fifty-six cases, in which, he says, the day of fruitful intercourse was known, there are only four in which parturition certainly occurred on the 280th day. Obstetricians can only speak with propriety of an average duration. This is attainable by striking it from the largest collection of well-ascertained cases. This average is the nearest approximation that can be made to what may be called the natural period of gestation. The data afforded by Dr. Montgomery for arriving at this mean or average, or nearest accurate general statement of the interval between fruitful connection and parturition, the duration of pregnancy, are of different degrees of value.



The most trustworthy and valuable are undoubtedly those cases of pregnancy which date from a single coitus. They number twenty-five, and their respective durations are as follows:—263, 264, 265, 265, 267, 270, 271, 272, 273, 274, 274, 274, 274, 274, 275, 275, 276, 276, 275 or 277, 277, 278, 280, 280, 287, 291 to 293. Of these twenty-five cases the mean is 274 days. The best data accessible to Dr. Montgomery, then, give 274 days as the duration of pregnancy, not 280.

It appears to me that the next most valuable data for settling this point are to be found in his table of thirteen cases dating from the day of marriage. The intervals between marriage and parturition in these cases were as follows:—261, 265, 268, 269, 270, 271, 271, 271, 272, 273, 274, 279, 291. In regard to these Dr. Montgomery himself says:—"The average interval between the day of marriage and that of labour was 272 days q. p., or thirty-nine weeks, minus one day; or, if we deduct the last case, which went to 291 days, the average interval would be  $270\frac{1}{3}$  days." Where, then, one naturally exclaims, are the grounds for saying that the natural duration of pregnancy is 280 days? This group of cases is interesting as including only primiparæ, a circumstance which probably accounts for the special shortness of pregnancy in them.

Dr. Montgomery's work presents us with another table of data. It consists of fifty-six cases, in which, he says, the day of fruitful intercourse was known. Now, to us, this table, at first sight, and before esti-



mating the results of it, appears to be of less value than either of the two former. Every case, almost, is invalidated because we do not know the authority or grounds upon which it is said that the day of fruitful intercourse was known. We do not know even the observers' names. Dr. Montgomery has laboriously collected cases of protracted pregnancy, all of which, so far as available for this table, find place in it. The whole weight and importance of it is contributed by the distinguished obstetrician's name that publishes it. That authority is undoubtedly of the very highest, but can scarcely be communicable to cases derived from a promiscuous set of observers, whose reasons for decidedly fixing on a single day are not given. In an exact investigation like this all cases should be rejected except those dating from a single coitus on a single day. But let us examine and see what this table affords towards the solution of the question. Omitting six cases where a single day is not given, we have fifty where the interval between fruitful intercourse and parturition is said to be as follows:—242, 258, 258, 263, 265, 267, 267, 267, 267, 268, 269, 269, 272, 273, 273, 274, 274, 275, 275, 276, 277, 277, 278, 278, 279, 279, 279, 279, 280, 280, 280, 280, 281, 283, 283, 284, 285, 286, 287, 287, 287, 288, 290, 291, 291, 292, 293, 293, 297. Of these fifty cases, all those satisfactorily known to Dr. Montgomery, the mean duration is 277 days. This table, framed under the conditions above described, yields a result opposed to the dogma of its author. Where, then, is the authority for stating



280 days as the natural period of gestation? It is nowhere.

We agree with Dr. Montgomery in his opinion that there is no other satisfactory method of arriving at the solution of this question but the one we have just followed—viz. the collection of well-ascertained facts and their analysis. “Independently (says he) of the very few cases in which we have satisfactory evidence of conception following casual intercourse, or perhaps a single coitus, we have no certain means of knowing exactly the commencement of gestation, and are obliged to form our calculation on one or other of three very fallacious grounds;” which he then proceeds to consider.

In the vast majority of cases the calculation of the day of confinement must be made from the termination of the last menstruation, for reasons which are well known. The average time to which a woman goes, after the last appearance of the menses, is 278 days (a period shorter than Dr. Montgomery's duration of pregnancy). This average is obtained by the collection of single observations and their subsequent analysis. If, then, we wish to ascertain the most probable day of a woman's confinement, we add 278 days to the last day of the last menstruation. The method of doing this, without a periodoscope, I have already shown.

Dr. Montgomery gives no specific directions for making this important calculation. But it appears from some passages occurring incidentally in his essay, that he adopts the following plan. Some day is selected



after last menstruation as the most probable day of fruitful intercourse, and 280 days are added thereto. As the selection of this day must be, in almost every case, made on the most worthless and insufficient grounds, the resulting calculation must be similarly characterised. Besides, if there be any truth in the statistical data of Dr. Montgomery, and their analysis given above, which is partly his own, then this plan of his must lead to a putting off of the probable day of confinement to far too distant a time. For instance, we have in the table of observations dated from the day of marriage, thirteen cases on Dr. Montgomery's own authority. Now, in these, as already stated, the women went on an average only 272 days from the day of the nuptials. If a probable day of fruitful intercourse after marriage had been selected, and 280 days added thereto, in these cases, such a plan would have evidently led to a mass of errors in the way of putting off the predicted day of confinement far too long.

I may here mention that with the subject of this important calculation or prediction Dr. Montgomery has confounded the question of the interval between insemination and conception. If such an interval existed, he says, "we should have no means of calculating the period of gestation with anything like an approximation to accuracy in any case." Now, if there be an interval in nature between insemination and conception we must adopt it, whatever results it may lead to. If it truly exist, it can lead only to true and good results. It is not considered probable by Dr.



Montgomery that any interval, or an interval of any importance, does exist. The highest authorities, however, on such a point are unquestionably very strongly in favour of the belief in its existence and its being of considerable extent, say several days. But in truth this question of a possible interval between insemination and conception has nought to do with the calculation of the date of confinement. Its truth or untruth does not affect such calculations, and no author but Dr. Montgomery has, so far as I know, discussed the two points as connected with one another in any way tending to modify practical precepts.



## CHAPTER VII.

## HARVEY'S OPINIONS.

GREAT men often seem to arrive at the truth, even in circumstances of complication and difficulty, by some process so simple that it appears like an operation of instinct. The immortal Harvey's expressed opinions in regard to the duration of pregnancy, and the calculation of the day of confinement, bear this character, for we cannot discover the grounds on which he arrived at results so nearly identical with those of modern science.

The interval between the festival of the Annunciation and the day of the blessed Nativity is that adopted by Harvey as unquestionably the ordinary term of utero-gestation. This is a period of 275 days, Lady-day, or the festival of the Annunciation being on the 25th of March, or 84th day of the year, while the day of the Nativity is the 25th December, or 360th day of the year. It is remarkable that the largest recent collection of cases made on certain or on the best grounds give also an average result of 275 days. Harvey, it will be observed, does not speak of any natural term, but only of the ordinary term, his correct appreciation of which is clearly indicated.



Harvey guards also his rule for calculating the day of confinement from being considered exact by saying that those prudent matrons who follow it "are rarely out of their reckoning." His statement is that after ten lunar months have elapsed from the commencement or appearance of last menstruation, they fall in labour the very day the catamenia would have appeared had impregnation not taken place. If the usual or average computation of the menstrual periods and intervals is adopted, the period of Harvey is 280 days, including the number of days of the last period. Ten times the usual interval and period of discharge—that is, ten times 28—gives 280 days; but as this includes the last period, of course the three, four, or five days of that period have to be taken from the 280 days, if we wish to find the interval he allowed between the end of last menses and parturition. Thus Harvey gives prudent matrons only an approximative calculation. The interval between last menstruation and parturition, according to him, is something a few days less than 280. The average time found by modern calculations, as stated in a former chapter of this part, is 278 days, with which Harvey's rules are as nearly in accordance as can be expected in a subject altogether incapable of any exact statement.

Dr. Montgomery's objections to Harvey's opinions are founded on the assumed accuracy of his own natural period of pregnancy—namely, 280 days after conception. We have already shown that this period



is assumed on insufficient grounds, and that as the day of conception is never known, we must seek some other method of calculating the day of confinement than any founded on the supposed day of such an occurrence. Without seeking to disparage the very high value and authority of Dr. Montgomery's writings, we publish these comments, believing that they demand modification of the views enunciated in his essay on the period of human gestation.

In conclusion, we venture to state the following propositions :—

1. That the interval between conception and parturition (the real duration of pregnancy) has not been exactly ascertained in any case.

2. That the average interval between insemination and parturition (commonly called the duration of pregnancy) is 275 days.

3. That the average interval between the end of menstruation and parturition is 278 days.

4. That the intervals between insemination and parturition, and between menstruation and parturition, have no standard length, but vary within certain limits.

5. That while absolute proof of the prolongation of real pregnancy beyond its usual limits is still deficient, yet there is evidence to establish the probability that it may be protracted beyond such limits to the extent of three or four weeks, or even longer.



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